What Works for Working Mothers? A Regular Schedule Lowers the Child Penalty*

Ludovica Ciasullo Martina Uccioli

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Abstract

Which working arrangements do mothers prefer, and how do these working arrangements affect the child penalty they experience? The Australian 2009 Fair Work Act explicitly entitled parents of young children to request a (reasonable) change in working arrangements. Leveraging variation in the timing of the law, timing of childbirth, and the bite of the law across different occupations and industries, we establish two main results. First, if allowed to request a change in working arrangements, new mothers ask for regularity in their schedule. Second, with regular schedules, working mothers' child penalty declined from a 47 percent drop in hours worked to a 40 percent drop. For the most exposed mothers, the Fair Work Act led to both a doubling in schedule regularity, and a 30% decrease in the child penalty in hours of work.

*Ciasullo: NYU, lc4177@nyu.edu; Uccioli: MIT, muccioli@mit.edu.

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1 Introduction

In virtually all countries in the world, women experience a large drop in labor supply and labor earnings when becoming parents, while men do not (Kleven, 2022; Kleven et al., 2022). Although this child penalty today accounts for a very large fraction of the gender earnings gap in many developed countries,[1] existing research has not fully identified its causes yet. Many potential explanations have been tested, none of which can completely explain the observed patterns. These explanations broadly fall in two categories: individual characteristics (biology, comparative advantage, norms)[2] and lack of direct parental support from government (such as paid parental leave, childcare)[3]; however, the role of the workplace that awaits for mothers as they return to work has received less attention.

Previous literature suggests that working arrangements are key in determining maternal labor supply (e.g. Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016), and policy makers seem to hold a similar belief. For example, as of August 2022 all European Union member states must apply the "Directive on work-life balance for parents and carers",[4] which establishes rights "such as the right to request flexible working arrangements, which will help people develop their careers and family life without having to sacrifice either"[5]; at the same time, the US Congress is debating the "Schedules That Work Act",[6] introduced on the premise that "Employees across the nation are forced to juggle the dual demands of home and work. However, too few workplaces provide work schedules that allow their employees to succeed at both."[7] Despite this widespread belief, we lack causal evidence linking working arrangements with the child penalty, likely because it is hard to find natural experiments that exogenously shift working arrangements. This paper represents a first step towards filling this gap.

In this paper, we exploit the Fair Work Act, a law introduced in Australia in 2009 that explicitly entitled parents of young children to request a change in working arrangements. We show two main results: first, if allowed to request a change in working arrangements, new mothers ask for predictability in their schedule; second, if granted predictability, they work more and their child penalty in labor supply is smaller than it would have been otherwise.

Working arrangements are what determines when, where and how a job is expected to be performed. For example, working arrangements include whether a worker has a predictable schedule or is on call; whether the job has to be performed in a specific location, such as the office or the home or a combination of the two; whether hours of work are

[1] For example, the child penalty is estimated to account for 70% of gender earnings gap in the 2010s in the US (Cortes and Pan, Forthcoming), 80% in Denmark (Kleven et al., 2019), 80% in Austria (Kleven et al., 2021a).

[2] e.g. in Kleven et al. (2021b); Andresen and Nix (2022); Kleven (2022); Boelmann et al. (2021)

[3] e.g. in Schönberg and Ludsteck (2014); Havnes and Mogstad (2011); Kleven et al. (2021a)

[4] Directive (EU) 2019/1158 of the European Parliament and the Council, link.

[5] Source: European Commission press release, 2 August 2022. url: `https://ec.europa.eu/commission/presscorner/detail/en/IP_22_4785`

[6] House bill H.R.6670, reintroduced on 02/09/2022 (originally introduced in 2015), link.

[7] Source: Congresswoman Rosa DeLauro (CT-03) press release, 20 June 2017. url: `https://delauro.house.gov/media-center/press-releases/delauro-and-warren-lead-members-reintroducing-schedules-work-act`

rigid or can be modified at the employees' will; whether one person's full time job can be carried out by two people working part-time.

The Australian Fair Work Act entitled parents of children below school age to request a change in working arrangements, thus creating the setting for two related but distinct pieces of analysis. First, the wording of the law is deliberately vague: it does not specify which working arrangements parents are entitled to request changes to. This allows us to study in a revealed-preference fashion which working arrangements parents choose to change when allowed to do so, and which ones they do not. Second, this law acts as a shifter to a certain subset of working arrangements, and we can use this natural experiment as quasi-exogenous variation to shed light on the effect of relaxing constraints on working arrangements on maternal labor supply.

The *Household, Income and Labor Dynamics in Australia* survey allows us to study the effect of workplace arrangements on the child penalty thanks to the richness of the information provided. In this longitudinal survey started in 2001, we can directly measure many working arrangements, through questions on scheduling (e.g. work Monday to Friday, on weekends, vary from week to week) and shifts (e.g. work on regular daytime schedules, in the evenings, on call), as well as workplace entitlements (e.g. entitlement to flexible start and finish times) and direct questions on working arrangements (e.g. hours of work carried out at home). We can also observe when children are born, and in each year whether and how many hours the parents worked and for what pay, thus allowing us to measure the child penalty in labor supply and earnings.

Using a flexible differences-in-differences framework, we show that the 2009 Fair Work Act did impact a specific type of working arrangements for new mothers: it increased predictability. Although the law is gender-neutral, and encompasses parents of children below school age – it expanded in 2013 to include school-aged children – we posit that its effect might differ by gender, parity, by age of the child, and over time, and we allow for such treatment effect heterogeneity. We find that women who became mothers after the passage of the law experienced a sharp increase in scheduling predictability, measured as working a regular daytime schedule from Monday to Friday, and that this increase was persistent. This also holds if we measure predictability simply as working a regular daytime schedule, as not working on weekends, and as not being on call. Instead, we do not find significant effects on other working arrangements, including flexible start and finish times, and results regarding working from home are mixed. We do not find any significant effect for men.

Since the difference-in-differences analysis above, and some of the analysis on the child penalty that follows, rely on the assumption that mothers who have their first child earlier in the sample are comparable to mothers who have their first child later in the sample, we provide two pieces of evidence in support of this assumption. First, we show that the fraction of women who become mothers any given year does not appear to change systematically around 2009. Second, we test whether pre-motherhood characteristics differ systematically between women who have their first child before or after 2009, and show that the two groups are balanced on observables.

We then turn to our main outcome of interest, the child penalty in labor supply, which at baseline is large for women and very small for men. The child penalty is defined as the change in a variable in the years following the birth of one's first child relative to before.

We show that at baseline women experience large and persistent child penalties in the labor market, while men don't: in the five years following the birth of their first child, on average women face a drop in labor force participation by 39 percentage points, while the corresponding figure for men is 2 percentage points. Even new mothers who continue working work on average 18 fewer hours per week, a figure driven by the fact that about two thirds of them switch from full-time to part-time work after giving birth to their first child.

However, the child penalty in both intensive and extensive labor supply decreased systematically for women who become mothers after 2009, suggesting a role of the change in working arrangements brought about by the Fair Work Act in mothers' labor supply decisions. We compare early cohorts of mothers, whose first child was born before 2009, and late cohorts of mothers, whose first child was born after 2009. Both on the extensive, but especially on the intensive margin, we observe an upward shift of maternal labor supply trajectories: both cohorts of mothers show a drop in labor supply after childbirth, but the drop is smaller at all horizons for the late one. The difference is sizable: in the five years following the birth of the first child, the drop in labor force participation is 7 percentage points smaller for late relative to early cohorts (from 39 to 32 percentage points) and the drop in weekly hours is 3 hours smaller (from a drop of 18 hours per week to one of 15 hours per week).

Comparing early and late cohorts of mothers is suggestive, but could be capturing a time trend: by estimating the child penalty in labor supply separately by year of birth of the first child, we show that there is a sharp, discontinuous, and robust change in 2009, in particular for the intensive margin. To argue this, we split women into cohorts defined by the year in which they become mothers, and for each cohort we compute the child penalty in labor supply in the first five years from childbirth. When we plot these child penalties against the year of first childbirth, we see a relatively stable trend in the years leading up to 2009, and then a very sharp discontinuity in the direction of greater labor supply (smaller child penalty) in 2009, which remains stable afterwards. We confirm with placebo tests and randomization inference that the break is indeed in 2009, especially for the child penalty in intensive margin labor supply (weekly hours and log weekly hours of work).

In order to strengthen the claim that the increase in predictability brought about by the Fair Work Act caused the decrease in child penalty, we construct a measure of exposure to the law, based on the standard deviation of predictability within occupation-by-industry cells before 2009. The basic idea is the following: there are certain jobs for which schedules were already fully predictable before the passage of the law, and hence the law did not have any bite on them; on the other hand, there are some jobs that cannot be made predictable for technological reasons, and the law did not have bite on them either (the law allows parents to request a change in working arrangements, but this request can be denied "on reasonable business grounds"). The jobs for which we observe that, in the years before 2009, some people were working predictable schedules, and some people were not, are those where we infer there were no technological barriers to predictability and there was room for increasing it. Based on this, we use the within-job standard deviation of predictability as our measure of exposure to the law. To proxy for "jobs", we use (2-digit) industry-by-occupation cells, which strike a good balance between within-cell uniformity

and sample size.

Using this measure of exposure, we find that the increase in predictability and the mirror decrease in child penalty in intensive-margin labor supply are entirely concentrated in the group of women with above-median exposure to the law. We assign women the level of exposure of the occupation-by-industry they were in two years before childbirth, and we track the evolution over time of the child penalty, separately for the group of women who had above and below median exposure to the law. We find that the level of predictability only increased for the mothers more exposed to the law; correspondingly, the entire increase in intensive-margin labor supply (decrease in child penalty) observed around 2009 is concentrated in the more exposed group. The magnitude is sizeable: in the more exposed group, late cohorts experience a child penalty that is 30% smaller than early cohorts, while this figure is 5% (not statistically different from zero) for the less exposed group. This split, however, does not explain the change in child penalty in labor force participation, as the two groups evolve in parallel along this dimension.

A potential main confounder to our analysis is the 2010 introduction of government-sponsored Paid Parental Leave (PPL): we show that our results are robust to accounting for it. We leverage the fact that, before the introduction of PPL, about 55% of working women were eligible for employer-provided paid maternity leave, an entitlement that we can observe in our data: we make the assumption that these women were not treated by the introduction of PPL. We find that not only accounting for PPL does not change our exposure-based results described above, but also that PPL *per se* does not have a significant impact on the child penalty in labor supply. This result is unsurprising, given the pre-existing institutional setting featuring unpaid leave with job protection plus a "baby bonus" payment, and is in line with similar findings in other contexts (e.g. see review in Olivetti and Petrongolo, 2017, and literature review below).

Our main analysis focuses on the child penalty in labor supply for women, and how it changes when working arrangements change. But the 24-hours constraint to daily activities implies that labor supply choices are necessarily interdependent with other choices of time use. And the majority of parents in our sample are partnered, so the choice of either parent to spend more or less time with the baby and more or less time at work relaxes or tightens the other person's time and resource constraint. We conclude the paper by describing how, when their first child is born, women vastly cut back on hours of work and leisure to replace them with housework and time spent playing with the child, while men's routine is mostly unaltered except for a few hours spent playing with the child. We also document that the maternal labor supply increase following the FWA comes at the expense of housework but not of time spent playing with children, and that fathers only pick up a small portion of the slack in housework. This opens the path for further research (in progress) into the determinants of intra-household allocation of tasks around parenthood and to shed further light on the causes of the child penalty.

Related literature This work is at the intersection of a large and growing literature on the determinants of the child penalty, and a recent but well established literature on the role of workplace arrangements on the gender earnings gaps.

Contrary to common wisdom, the size of the child penalty does not seem to be affected

by parental leave policies (Schönberg and Ludsteck, 2014; Dahl et al., 2016; Kleven et al., 2021a), while the evidence on childcare is mixed (positive in Gelbach, 2002 and Lefebvre and Merrigan, 2008, null in Havnes and Mogstad, 2011 and Kleven et al., 2021a).[8] Similarly, neither biology nor comparative advantage are able to explain the drop in labor earnings women – and not men – face when becoming parents (Andresen and Nix, 2022; Kleven et al., 2021b). Instead, gender norms seem to be a strong predictor of the size of the child penalty: Boelmann et al. (2021) show that, when moving to west Germany, eastern German women born between 1946 and 1994 have a lower child penalty than their new neighbours, carrying with them east Germany pro-work attitudes; Kleven (2022) shows that child penalties correlate strongly with measures of gender progressivity, both over time and across countries and US states, and that, when moving to the US or within the US, migrants experience child penalties that are strongly correlated with those of their country or state of origin. For a recent review of the literature on the child penalty, see Cortes and Pan (Forthcoming).

We contribute to this literature by focusing on the role played by the characteristics of the workplace that women face upon returning to work, and more specifically flexibility and scheduling predictability, which have been shown to be a key determinant in gender earnings gap (Bertrand et al., 2010; Goldin, 2014; Goldin and Katz, 2016). Two papers we are aware of have addressed a similar question: Bütikofer et al. (2018) compute child penalties for different professions in Norway for mothers who have their first birth between 1989 and 2000, and show that the child penalty is smaller in more flexible professions; Bang (2021) shows that child penalties in the US between 1979-2016 are decreasing in own and spouse's flexibility. We build on and complement these descriptive patterns by leveraging an exogenous change in workplace arrangements to establish two novel facts: first, when given the possibility to change working arrangements, Australian mothers choose to change predictability (which can be considered a form of flexibility, in Goldin, 2014's definition of the term that both these papers build on); second, that there is indeed a causal link between predictability and the size of the child penalty.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the data. Section 3 presents the "first stage": it shows that the Fair Work Act shifted some working arrangements (predictability) but not others (flexibility). Section 4 formally defines the child penalty, our main object of interest, and documents its evolution over the last twenty years in Australia, showing a discontinuous decrease around 2009 - the "reduced form". Section 5 ties together the previous two sections, by showing that the decrease in child penalty is entirely concentrated in the group of mothers in occupations most exposed to the changes in predictability brought by the Fair Work Act. Section 6 rules out the role of Paid Parental Leave as a potential confounder. Section 7 introduces future work on intra-household allocation of time and tasks around parenthood leveraging this setting and data. Section 8 concludes.

[8]The effects of family policies on female labor market outcomes are reviewed in Olivetti and Petrongolo (2017).

2 Background and setting

The Australian 2009 Fair Work Act (FWA) entitled parents of young children to request alternative working arrangements. Working arrangements are a multidimensional concept, spanning from the possibility of working from home to the definition of the working schedule, and more. To be more precise, by working arrangements we indicate the job characteristics that define when and where a job is expected to be performed. The Fair Work Act represents a quasi-exogenous shift in ability to change working arrangements, which we will leverage both to study which working arrangements mothers seek to change, and to make causal statements on the link between working arrangements and the child penalty.

2.1 Institutions: the Fair Work Act (2009)

The Australian 2009 Fair Work Act establishes, among other things, that parents of children under school age are entitled to request a "change in working arrangements" and employers can refuse only "on reasonable business ground" (Fair Work Act 2009, Act No. 28, Section 65). The legislator provides examples of the requests that can be made under the scope of this law, such as the adoption of an 8-to-4 workday as opposed to a 9-to-5 one, or a split of the 35 hours per week in 4 days rather than 5[9]. However, the language is quite general, and does not restrict to a specific type of working arrangement: Section 3 of this paper studies precisely *which* working arrangements were impacted by the passage of this law. Examples of "reasonable business ground" that can lead to denied requests include the requested arrangements being too costly or that other employees' working arrangements cannot be changed to accommodate the request.[10]

This law, which has been fairly salient and present in the public discourse,[11] includes several provisions aimed at making work "more fair", but the greatest majority of them apply to all workers and not just to parents. In our analysis, we leverage the parents-non parents comparison to control for more general labor market impacts of this legislation. The other provisions include: defining 38 hours a week the maximum weekly hours, above which an employee has to be paid overtime pay; establishing an entitlement of 4 weeks per year of paid annual leave; setting a minimum number of days of notice before firing. The law also extends *unpaid* parental leave from 12 to 24 months, but take-up for this provision was low (O'Neill, 2012), and, if anything, we would expect it to play against our results (Olivetti and Petrongolo, 2017). Paid Parental Leave, which was introduced the following year, is discussed in detail in Section 6.

With the Fair Work Amendment Act of 2013, the right to request a change in working arrangements was extended to parents of all school-aged children – the flexible specification we employ in our analysis will account for this.

[9] Examples from the government-run Fair Work website: `https://www.fairwork.gov.au/tools-and-resources/best-practice-guides/flexible-working-arrangements`

[10] Fair Work Website, see footnote 9

[11] According to Google searches, interest for this law is substantially larger than that for other labor-related queries such as "Trade Unions", "Unfair dismissal", or "Disability Discrimination Act" – see Appendix Figure A.1.

2.2 Data: HILDA

The Household, Income and Labour Dynamics in Australia (HILDA) Survey covers a representative panel of the Australian population and combines information on family and work, including hours worked as well as workplace entitlements. This rich dataset allows us to identify people who become parents as well as their occupations (and hence their exposure to the Fair Work Act) and their labor supply around childbirth and over time.

HILDA is a household-based ongoing longitudinal survey, started in 2001, designed to be representative of the Australian population. For each of the approximately 8,000 sampled household, everyone above 15 years old is interviewed, but basic demographics are also collected for younger members of the household. This gives us information on family structure, arrival of new babies, and partners (both married couples and *de facto* partners). At the time of the analysis, data up to 2019 are available to researchers – hence our sample covers the years 2001-2019.

Variables construction HILDA asks a variety of questions on workplace entitlements, job characteristics, scheduling, and job location that we can use to measure working arrangements. We primarily focus on three types of working arrangements: scheduling predictability, flexibility, and work from home. Our main measure of predictability is called "Regular schedule" and is a dummy equal to one if the respondent has a Monday to Friday weekly schedule and a regular daytime schedule. We occasionally refer separately to the variables "Regular weekly schedule (Monday to Friday)" and "Regular daytime schedule", which jointly determine our main measure; and to "On call", which is a dummy equal to one if the respondent reports working on call or irregular shifts. We measure flexibility with two different variables: the first is a dummy indicating agreement with the statement "My working times an be flexible", the other one is a dummy for whether the employee reports being entitled to flexible start and finish times. Finally, respondents are asked how many hours a week they work from home and also whether they would be entitled to home-based work if they wanted it, and we use both measures. Details on variable construction can be found in Appendix B.

Sample restrictions We restrict our sample to people aged 15 to 65. Since our analysis focuses on how outcomes evolve around childbirth, from Section 4 onward we only keep parents that have their first child between 2003 and 2017, in order to observe at least two years before and after childbirth. For these parents, we only keep observations in the range [-6,+10] years relative to the first childbirth, in line with Kleven et al. (2019). In studying the effect of the FWA on the child penalty, when we compare early and late cohorts (non-treated and treated by the FWA, respectively), we exclude the cohort of parents who have their first child exactly in 2009, since this cohort is partially treated. We keep non-parents and individuals who don't have a first child during the sample years, and they act as controls in all our specifications.

Descriptive statistics Table 1 shows the basic summary statistics, for our entire sample, split by gender. Table 2 zooms in to will-be mothers, and presents a snapshot two years

before childbirth. Appendix Table A.1 shows the distribution of working mothers, again two years before childbirth, across the different occupations and industries.

Australian mothers in our sample have a fairly strong labor market presence before childbirth: two years before childbirth, 93% of them are in the labor force, working roughly 39 hours per week. Among the employed, 20% of them work part time, but almost half of them work over the standard full time amount of 38 hours per week and roughly 10% over 50 hours per week. More than 70% of those who work have a permanent contract (as opposed to being a casual or fixed-term employee); about half of them are in supervisory roles and 18% of them received a promotion in the previous year. Along all of these dimensions, will-be mothers are roughly comparable to men in the full sample.[12] In Section 4, we will see that all these numbers change dramatically once they have their first child.

Will-be mothers are on average more educated than the full sample, work on slightly more predictable schedules and have marginally less flexible jobs. 38% of will-be mothers have a bachelor degree, as opposed to 27% for the full sample of women and 22% for the full sample of men. Roughly 60% of women work on a regular Monday to Friday daytime schedule two years before their first childbirth, while on average 42% of women and 53% of men do, and only 6% of will-be mothers reports working on call and irregular shifts, while this figure is 10% for both men and women in the full sample. While about 40% of working women agree with the statement "My working times can be flexible" two years before becoming mothers, this is lower than the 50% figure for the full sample.

3 "First stage": the Fair Work Act (2009) shifted working arrangements but did not affect fertility

In this section, we show that the Fair Work Act did have bite, and in a specific way: it increased scheduling predictability, but not flexibility, for a subset of treated mothers; thus representing an exogenous shift in a specific working arrangement we will leverage in subsequent analysis. We also show that the law did not observably affect fertility or selection into motherhood, thus supporting the assumption that women becoming mothers before and after the law can be meaningfully compared.

3.1 The effect of the Fair Work Act on working arrangements

3.1.1 Empirical strategy

Given the wording of the law, the most natural way to study the effect of the 2009 Fair Work Act is through a standard difference-in-differences design, but we want to allow for treatment effect heterogeneity. The FWA entitles parents of children under school age to request a change in working arrangements, and thus the most natural specification would be one that includes all parents of children who are younger than five in the treated

[12]The most notable differences are that men in the full sample are twice as likely to work more than 50 hours a week, and half as likely to be promoted – some of it could also be driven by the different age composition of will-be mothers versus the full sample of men.

group (after 2009) and everybody else, both non-parents and parents of older children, in the control group. However, we have the prior that the law would impact different demographics differently: for example, if women are those who disproportionately care for the children, a law which is explicitly aimed at improving work-family balance[13] might disproportionately affect them. Similarly, we might hold the prior of path dependence in maternal labor supply: we could think that labor supply choices are made within the first year from the birth of the first child, and then re-optimized only in case of large changes to the environment. In our main specification we allow for enough flexibility to account and test for those priors.

We run the following specification, separately by gender:

$$\begin{aligned} Y_{it} = & \sum_{a \in A} \left(\sum_{j \neq 2009} \beta_{FC_a,j} \times \mathbb{1}\{FC(i,t) \in a\} \times \mathbb{1}\{t = j\} + \beta_{FC_a} \times \mathbb{1}\{FC(i,t) \in a\} \right) + \\ & + \sum_{a \in A} \left(\sum_{j \neq 2009} \beta_{YC_a,j} \times \mathbb{1}\{YC(i,t) \in a\} \times \mathbb{1}\{t = j\} + \beta_{YC_a} \times \mathbb{1}\{YC(i,t) \in a\} \right) + \\ & + \alpha_i + \delta_t + \gamma_{h(i)} + \epsilon_{it} \end{aligned} \tag{1}$$

where Y_{it} is the outcome of interest for individual i at time t, $\mathbb{1}\{FC(i,t) \in a\}$ is an indicator function that takes the value of one if i's *first* (oldest) child in year t is in age range a, and $\mathbb{1}\{YC(i,t) \in a\}$ is an analogous function but based on the age of the *youngest* child. The age ranges $a \in A$ used in the estimation are 0-2, 3-5, 6-8, 9-10, 10+. $\mathbb{1}\{t = j\}$ is an indicator function for the year being j, and $\alpha_i, \delta_t, \gamma_{h(i)}$ are individual, time and age fixed effects respectively. The coefficients of interest are the $\beta_{FC_a,j}$'s and the $\beta_{YC_a,j}$'s, which tell us how differently at any point in time parents of first/youngest children in age range a behave from analogous parents in 2009 (controlling for individual, time and age of the parent fixed effects).

The underlying assumption for the validity of this analysis is that individuals who choose to become parents at different points over the sample period are comparable: we show evidence in support of this claim for mothers (on whom our analysis focuses) in Section 3.2.

3.1.2 Results

Predictability increases for mothers The main result of the strategy just described is summarized in Figure 1. In this figure, we plot the estimated $\beta_{FC_a,j}$'s and $\beta_{YC_a,j}$'s from equation (1) where the outcome variable is "Regular Schedule", a dummy for working from Monday to Friday on a regular daytime schedule[14], and in which we restrict the sample to only women. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_a,j}$'s, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_a,j}$'s, for

[13] Section 3 of the Law describes its objective as "to provide a balanced framework for cooperative and productive workplace relations that promotes national economic prosperity and social inclusion for all Australians by [...] assisting employees to balance their work and family responsibilities by providing for flexible working arrangements"

[14] More details on variable construction are in Section 2.2 and Appendix B.

child's age ranges 0-2, 3-5, 6-8. The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and so on).

The fraction of mothers on regular schedules increases substantially after 2009, but with considerable heterogeneity according to age and parity of the child. From the first panel in Figure 1 we learn that, in 2009, 26% of working mothers who had given birth to their oldest child in the previous two years worked on a regular schedule, and this figure is similar for new mothers of 0-2 year old children in the preceding years. Starting in 2010, this fraction starts increasing by 10 and up 20 percentage points, constantly remaining at a higher level afterwards. In the second panel of the top row, where we display coefficients on the dummy for the oldest child being between 3 and 5 years old, we notice a very similar pattern, but with the increase starting after 2013, the year in which the first children born after the passage of the Fair Work Act turned three. Remarkably, in the third panel, which displays coefficients on the dummy for the oldest child being between 6 and 8 years old, a series of estimated zeros is followed by a sharp and permanent increase, but only after 2016, when the first post-FWA children turned six.

While we observe an increase in predictability for mothers whose first child was born after the passage of the FWA, this does not appear to be the case when we split mothers based on the age of the youngest child. The bottom panels in Figure 1 mirror the top panels, but they display the estimated coefficients on the dummies based on the age of the youngest child. In all three panels, all the coefficients estimated are statistically indistinguishable from zero at all horizons.

Predictability increases for mothers, flexibility does not and working from home is unclear Informed by the patterns just described, and in order to gain precision and facilitate comparisons, we estimate a restricted version of equation (1), where we only include the dummies based on the age of the first child, and we replace the year-by-year interactions with mobile "post" dummies. In particular, we interact the dummy for the first child being between 0 and 2 with an indicator for the year being equal or after 2010; the dummy for the first child being between 3 and 5 with an indicator for the year being equal or after 2013; the dummy for the first child being between 6 and 8 with an indicator for the year being equal or after 2016. These mobile "post" dummies are meant to capture the point at which mothers whose first child was born after the passage FWA start entering the indicated group, in line with the evidence presented above.

The first four columns of Table 3 confirm that the result on predictability holds regardless of how we define it: whether we define it as Regular schedule (column 1), as either of its two components - Monday to Friday schedule (column 2) or Regular daytime schedule (column 3), or as the inverse of being on call (column 4). The table also allows us to gauge magnitudes: after the passage of the law, "Regular schedule" increases by almost 40% among mothers whose first child is between 0-2 years old relative to their earlier counterpart, driven both by an increase in Monday to Friday scheduling by 33%, and increase in regular daytime schedule by 10% .

Flexibility, which was explicitly targeted by the Fair Work Act, does not appear to have been affected by it, at least on average. Columns 5 and 6 of Table 3, which report the coef-

ficient on the restricted specification above for the outcome variables indicating agreement with the statements "My working times can be flexible" (column 5) and "I am entitled to flexible start and finish times" (column 6), display coefficients that are small and mostly not significantly different from zero. Appendix Figure A.2, which replicates Figure 1 but using agreement with the statement "My working times can be flexible" as outcome variable, confirms that this estimated zero is not an artifact of the restricted specification, and that flexibility does not seem to be changing for mothers of young children around the passage of the Fair Work Act.

The analysis reveals a mixed picture regarding work from home. The entitlement to be working from home – which is the only aspect that the law can directly target – didn't significantly change (column 10 of Table 3). Actual hours worked from home (column 7) increased by about 0.5-1 hours per week, with differing combinations of changes on the extensive margin (fraction of women doing any work from home, column 8) and on the intensive margin (hours worked from home conditional on working from home at all, column 9) across child's age group. Appendix Figure A.3 makes this mixed picture visually clear: not much seems to be happening to mothers of a 0-2 year old child in terms of hours worked from home; mothers of whose first child is between 3 and 5 do experience an increase in hours worked from home, after 2013, but it is noisy and short-lived; mothers of 6-8 year old first children follow the same pattern, but shifted by 3 years. So we cannot say with confidence whether the Fair Work Act impacted work from home.

No working arrangement changes for fathers We replicate the same analysis on men, and show that nothing seems to be happening for fathers of young children around 2009. Appendix Figure A.4 replicates Figure 1 for predictability on the sample of men. We notice that a much larger percentage of working fathers than of mothers tend to have a regular schedule (60%), and that this fraction does not show any systematic changes, neither when we compare fathers based on the age of their first child (top row), nor when we do it based on the age of their youngest child (bottom row). All panels show long lines of estimated zeros at all horizons. Appendix Table A.2, which replicates Table 3 on the sample of men, shows that nothing changes for fathers around 2009 also with respect of the other working arrangements considered.

Interpretation We interpret the evidence as supporting a few key facts, which inform the subsequent analysis. First, the Fair Work Act has had a statistically significant impact only on the working arrangements of mothers, and none on those of fathers. This is consistent with government reports, which indicate that the greatest majority of requests made under the Fair Work Act come from women (76% of them, according to survey evidence in O'Neill, 2012). This insight, together with evidence we will present in Section 4.1 of the child penalty being a female-only issue, motivates our focus on women. Secondly, we show that the entirety of the effect is concentrated among women whose first child was born after the passage of the law, and was persistent. This suggests the following pattern: women rearrange their working life and re-optimize soon after they become mothers, based on the constraints they face in that moment, and this arrangements are sticky; so that mothers of three year old children, although technically treated by the law, did not change their

arrangements when the FWA was introduced. This supports our choice in subsequent analysis of defining treatment and exposure to the law based on the year of birth of the first child.

3.2 The Fair Work Act did not observably alter fertility or selection into parenthood

In order to support the assumption that early and late cohorts of mothers (women becoming mothers before and after the passage of the FWA) can be meaningfully compared, we show that the fraction of women having their first child each year (fertility rate) did not change around 2009, and that the two groups of mothers are similar on observable characteristics measured two years before childbirth.

Australian women did not become mothers at different rates around 2009. Figure 2 plots the fraction of Australian women in our sample (aged between 15 and 65) who give birth to their first child by year, and superimposes best-fit lines separately before and after 2009. Although there is a slight upward trend in first births, it does not seem to change systematically around 2009, supporting the claim that the law did not have a direct impact on rates of first births. The graph looks very similar if we restrict the sample to women aged 20 to 45 (although the range of the y-axis understandably changes).

Women who became mothers before and after 2009 were not observationally different when compared before childbirth. Table 2 presents means and standard deviations of key labor market variables for mothers who had their first child between 2003 and 2008 (early cohorts) and for mothers who had their first child between 2010 and 2017 (late cohorts), measured two years before their first childbirth. The last two columns present the between-groups difference, and standard indications for whether the difference is significantly different from zero. None of the variables displays a significant difference across the two groups, except for year of first birth (by construction), and wage and income variables, which is explained by inflation. In Appendix Table A.1 we show that "early" and "late" mothers were also working in similar industries and occupations before childbirth.

4 "Reduced form": the child penalty in labor supply decreased following the Fair Work Act (2009)

In this section we define and compute the child penalty in labor supply, the change in labor supply following the birth of the first child relative to before, and show that it is large and negative for women and close to null for men. We further show that the child penalty in labor supply for women changed discontinuously around the passage of the Fair Work Act, in the direction of greater labor supply, suggesting a role of the law on maternal labor supply choices; in the next section we will show further evidence that this link is causal.

4.1 The child penalty at baseline

4.1.1 Definition

The child penalty is defined as the distance between actual outcomes of parents and the counterfactual that would have realized absent childbirth. More specifically, we define as "child penalty in Y, k years since childbirth" the estimates of coefficients γ_k from the following event study regression:

$$Y_{it} = \alpha_i + \delta_t + \beta_{h(i)} + \sum_{\substack{k=-5,\\ k\neq -2}}^{10} \Big\{\gamma_k \times \mathbb{1}\{t - E_i = k\}\Big\} + \epsilon_{it} \ , \tag{2}$$

where E_i represents the year of birth of i's first child, and $\alpha_i, \delta_t,$, and $\beta_{h(i)}$ are individual, time, and age fixed effects respectively. The coefficients of interest are the γ_k's, representing the effect on the outcome Y of being k periods from childbirth. Never-parents and individuals who don't have their first child in our sample years are kept in the sample and act as controls. In order to account for heterogeneous treatment effects across cohorts, we estimate the equation above using the Sun and Abraham (2020) estimator. We cluster standard errors at the individual level.

4.1.2 The child penalty in labor market outcomes by gender

The child penalty, as defined above, is gender neutral, and can be computed for any outcome of interest: here we focus on labor market outcomes, and we compute the child penalties separately by gender. Here we want to present baseline child penalties, net of any potential effect of the Fair Work Act, and hence we only estimate it on parents whose first child was born before 2009.

Labor supply and earnings Having a child is followed by a very large drop in labor supply and labor earnings for mothers, but by very little change for fathers. Figure 3 shows the estimates of the γ_k's from equation (2), in stereotypical blue for men and pink for women. Panel (a) shows that on the year of their first childbirth, women are 52 percentage points less likely to work, relatively to two years earlier (when 93% of them were working). Labor force participation recovers in the year following childbirth, but never fully: ten years after becoming mothers, women are still 30 percentage points less likely to be in the labor force, relatively to two years before childbirth and to non-parents. Even mothers who do remain in the labor force experience a large drop in labor supply: intensive-margin labor supply falls by 19 hours of work per week, among those who remain employed, and never recovers (panel (b)). The log of weekly hours spent in employment permanently drops by 0.8 (panel (a) in Appendix Figure A.5). The drop in labor supply corresponds to a proportional drop in labor income (panel (d)), given that hourly wages don't show systematic changes (panel (c)).[15] None of this happens to fathers, for whom the child penalty is tiny

[15]Hourly wage is computed as the ratio of weekly wage and weekly hours. Given the wording and placement in the survey of the questions priming these variables, there might be a mismatch in the reference week. This is unlikely to be a problem in general, but could be a problem the year of childbirth, when hours

at all horizons – the point estimates are very small in magnitude (the post-chilbirth drop is of about 2 percentage points on the extensive margin, an about 1 hour a week on the intensive margin labor supply), and most confidence intervals include zero, for all labor market outcomes considered.

This baseline analysis showing large child penalties for women in all labor market outcomes, and none for men, further motivates the focus on women in the rest of the paper.

A small aside on wording. The post-birth changes in labor supply that we estimate for women are large and negative; hence, according to the definition given above we should say that the child penalty in labor supply for women is negative. However, the word has always been used with the opposite meaning – "we observe a large child penalty in earnings for women" generally means that we observe a large *drop* in earnings after childbirth. We always try to be as specific as possible, but when comparing child penalties across groups this might result in awkward writing. In these instances, we will use "child penalty" in labor supply and earnings with the meaning it commonly has – to indicate a drop in labor supply and earnings.

Other labor market outcomes Child penalties in labor supply and earnings are a good summary of the main labor market changes faced by women as they become mothers, but our data allows us to paint a much richer picture. Appendix Figure A.5 expands on Figure 3 by including other labor market outcomes; panel A of Appendix Table A.5 summarizes the child penalties for women in a variety of labor market outcomes. From these exhibits, we learn that the observed drop in hours of work is driven by a *four-fold* increase in the fraction of women working part-time – an increase by more than 60 percentage points, off a pre-childbirth base of 22% – and by a zeroing of overtime work, both if measured as working more than the government-defined standard of 38 hours/week (a drop by about 40 percentage points, off a base of 44%) and if measured as working more than 50 hours/week (a drop by more than 10 percentage points, off a base of 11%).

Also the type of contract and the type of roles change. While in the years before childbirth, more than 70% of working women have a permanent contract, this figure drops by 20 and up to 30 percentage points in the years following the birth of the first child: even among the women who do retain employment, many of them switch from permanent to casual and fixed-term employment, and the fraction of women who do the switch increases as the child grows up (rather than stabilizing around year one, as most of the other labor market outcomes do). After becoming mothers, women are 30% less likely to be in a supervisory role (drop of about 15 pp, off a base of 50%) and half as likely to be promoted (drop of about 8 pp, off a base of 16%).

of work might approach zero, but the mother might still be receiving payments for work carried out int the past, or she might interpret maternity payments as wages. This is probably the cause for the spike in hourly wage we observe for women the year of first childbirth. As such, we interpret it as noise. We discuss this variable further in Appendix B.

Child penalty in working arrangements and job characteristics Many working arrangements change for women when they become mothers. Note that here we are doing an exercise that is different from what we presented in Section 3: there, we were discussing how mothers in certain years compared to mothers of similarly-aged children in previous years. Here, we are discussing how mother's working arrangements compare to those they themselves had before their first childbirth; focusing only on women who became mothers before 2009. Panel A of Appendix Table A.6 shows that mothers are substantially less likely to be in predictable jobs than their pre-motherhood selves (columns 1-4), that they are more likely to agree with the statement "my working times can be flexible" (column 5), and that they are more likely to work from home but for fewer hours, so that on average the number of hours worked from home doesn't change (column 7-9). The child penalty in some of these arrangements was affected by the Fair Work Act - panels B offers a preview, which we will discuss more in detail in later sections.

Mothers tend to work in more flexible jobs than their pre-motherhood selves, when we define job flexibility based on Goldin (2014). Panel A in Appendix Table A.7 displays the child penalty in different measures of inflexibility. The outcomes in columns 1 through 4 are variables based on the O*NET characteristics that underlie Goldin's measure of inflexibility: Time pressure, Importance of relationship, Unstructured work, Freedom to make decisions. "Inflexible job" in column 5 is the index of inflexibility that averages these four measures. We transpose these variables to HILDA using questions that get at these dimensions, in order to maintain time and within-occupation variation, but if we merge in Goldin's original measure from O*NET through occupation we get a similar picture. Appendix B describes the construction of these variables in detail; all the variables are measured in z-scores. All these measures of inflexibility of one's job decrease after the birth of the first child, suggesting that mothers make their jobs more flexible when they have a child to care for.

Focusing on specific job characteristics of "Freedom in decision making" and "Time pressure, complex and stressful job", as identified via principal component analysis, confirm the same story. The outcomes in columns 6 and 7 of Appendix Table A.7 are the first two components from carrying out Principal Component Analysis on the very large set of job characteristics we can observe. They are both described in Appendix B, and they are both measures in z-scores. "Freedom in decision making", in particular, is associated with managerial roles, while "Time pressure, complex and stressful job" is distributed similarly across different occupations. After the birth of their first child, women's job are characterized by one third standard deviation less freedom in decision making, and two thirds standard deviation less time pressure and stress.

A large part of the observed changes after motherhood come from within-job changes, but the across-job changes (arrangements and characteristics varying after childbirth relative to pre from changing job) are non-trivial, especially along certain dimensions. In Table A.8, we proxy jobs by occupation-by-industry-by-education cells (since we can't directly observe employer) and compute averages of the variables we are interested in studying by these cells. We then assign individuals these job averages, and compute the child penalty using as outcome these averages. While computing the child penalty directly on a given variable recovers a combination of across and within job changes, this exercise isolates the change coming exclusively from changing job: by comparing the two, we can learn how

much each component explains.

From comparing Table A.8 with tables A.6 and A.7, we learn that the largest part of the child penalty in working arrangements and job characteristics comes from within-job changes, and only a small fraction from actually changing occupation and/or industry, but this differs according to the specific variable under examination. For example, in column 1 we see that there is some decrease in predictability coming from moving to less predictable occupations and industries, but the estimated change from changing job is only about one tenth than the overall estimated child penalty in predictability. At the other end of the spectrum, for "Freedom in decision making" – usually associated with managerial occupations, about two thirds of the observed child penalty come from changing occupation or industry.

4.2 The reduction in child penalty following the Fair Work Act (2009)

Here we show that the child penalty in labor supply for mothers changed discontinuously after the passage of the Fair Work Act, in the direction of greater labor supply of new mothers. We do it in two ways: first, we compare the child penalty of early and late cohorts of mothers (who had their first child before and after 2009, respectively), and show that late cohorts of mothers experience a smaller child penalty in labor supply. Second, we compute the child penalty by year of birth of the first child, and we show that the change is sharp around 2009, rather than reflecting a time trend, and that this result is robust to permutation and randomization inference.

4.2.1 Early vs late cohorts: late cohorts experienced a smaller child penalty

Empirical strategy We start by estimating the child penalty for each year relative to childbirth, separately for early and late cohorts of mothers. In particular, we estimate equation (2) separately for the two groups, keeping the non-mothers as controls in both, and we plot the resulting estimates in the same graph, to visually see the difference.

We also estimate the child penalty for the two groups of cohorts jointly, in a more succinct specification, in order to formally test for their difference. In this specification, we group years relative to childbirth in 0 to 5 and 6 to 10: this preserves power, and it is justified in light of the previous results in Figures 3 and 4, which show great persistence in the child penalty for several years after the birth of the first child. Formally, we estimate the following model:

$$\begin{aligned} Y_{it} = \alpha_i + \delta_t + \beta_{h(i)} &+ \gamma_{0-5} \times \mathbb{1}\{(t - E_i) \in [0,5]\} + \\ &+ \gamma_{6-10} \times \mathbb{1}\{(t - E_i) \in [6,10]\} + \\ &+ \gamma_{0-5,post} \times \mathbb{1}\{(t - E_i) \in [0,5]\} \times \mathbb{1}\{E_i > 2009\} + \\ &+ \gamma_{6-10,post} \times \mathbb{1}\{(t - E_i) \in [6,10]\} \times \mathbb{1}\{E_i > 2009\} + \epsilon_{it} \end{aligned} \tag{3}$$

where $\mathbb{1}\{(t - E_i) \in [0,5]\}$ is an indicator function for i's first child being between 0 and 5 years old, $\mathbb{1}\{(t - E_i) \in [6,10]\}$ is an indicator function for i's first child being between 6 and 10 years old, and $\mathbb{1}\{E_i > 2009\}$ is an indicator function taking value of one if i's first child was born after 2009 – meaning, an indicator for i being part of the late cohorts. The 2009

cohort is partially treated, and we exclude it from this analysis. In this specification, the estimates of γ_{0-5} and γ_{6-10} summarize the child penalty in Y for the early cohorts, which we have discussed in Section 4.1; the object of interest and discussion in this section are $\gamma_{0-5,post}$ and $\gamma_{6-10,post}$, the differences in child penalty for late cohorts relative to early cohorts.

Main outcomes: Labor supply and earnings The year-on-year child penalties in labor supply for late cohorts display a parallel vertical shift upwards relative to that of early cohorts, in the direction of greater labor supply (smaller child penalties); this corresponds to a proportional smaller drop in earnings. We can see it in Figure 4, where green circles display the estimates of γ_k's from equation (2) estimated only on mothers whose first child was born between 2005 and 2008 (and non-mothers, as controls), and red squares display the same estimates but on the sample of mothers whose first child was born between 2010 and 2013. We show the results of formally testing for the difference in Table 4, where we report the coefficient estimates from equation (3). In this section, "panels" refers to Figure 4 and "columns" refer to Table 4.

In panel (a) we observe that later cohorts of women experience a drop in labor force participation upon childbirth similar to the one of the earlier cohorts, but recover faster and by a greater amount: on average, in the first five years after the birth of their first child, the drop in labor force participation is 7 percentage points smaller for later cohorts relative to earlier cohorts (corresponding to a 18% lower drop), and even in years 6 to 10 from the first childbirth, the gap between the cohorts is still of about 5 percentage points, suggesting persistence (although for longer horizons the coefficients are less precisely estimated). The precise numbers are in column 1.

On the intensive margin labor supply, the parallel vertical shift is even starker than on the extensive margin, as we can see from panel (b) and columns 2 and 3. The drop in weekly hours of work following childbirth for later cohorts is constant at about 15 hours/week in the first ten years from the birth of the child, a drop which is three hours smaller (18% smaller) than for earlier cohorts. In log terms, the drop for later cohorts is on average 0.15 smaller than the 0.8 drop of early cohorts. As for early cohorts, also for late cohorts hourly wages are not affected by childbirth (panel (c)), and hence the smaller drop in labor earnings faced by the late cohorts of mothers is proportional to the smaller drop in labor supply (panel (d)).

Other labor market outcomes The smaller drop in hours comes from both less part-time and more overtime, and is accompanied by very large changes on the contractual margins. In Appendix Figure A.6 and Appendix Table A.10 we can see that later cohorts experience a somewhat smaller increase in the share of mothers working part-time (57 pp as opposed to 61 pp, a 6% smaller increase, in the first five years from childbirth) but the difference is not statistically significant (although the average masks significant heterogeneity, as we will see in Section 5). Changes in overtime are larger and significant at the 5% level: the drop in overtime is 5 pp (13%) smaller for later cohorts at the 38 hours/week threshold, and it is 2.3 pp (22%) smaller at the 50 hours/week threshold. Contract type is where we see the largest changes. In early cohorts, we observed a shift from permanent to causal

and fixed-term employment by 23 percentage points in the first five years from the birth of the first child; for late cohorts, this number is less than 10 percentage point (pre-birth fractions of permanent contracts are similar for the two cohorts, at about 70%), suggesting substantial rearrangements.

Changes to child penalty in working arrangements In Section 3 we described how the Fair Work Act changed certain working arrangements for the mothers of young children, but only for mothers whose kids were born after the passage of the law. The same analysis can be recast as changes in the child penalty in these working arrangements: many working arrangement change systematically for women when they become mothers, but for women that became mothers after 2009 some of them changed systematically less relative to earlier cohorts of mothers. More specifically: Appendix Table A.11 shows that when women become mothers, they move to less predictable jobs (a decrease by 44 percentage points in the fraction of women working Monday to Friday, regular daytime schedule, for early cohorts), but this is systematically less true for later cohorts (36 percentage point, a 19% change), which is driven both by fewer women switching out of Monday to Friday jobs, and fewer women switching out from a regular daytime schedule and into on call and irregular shifts.

Other working arrangements do change when women have children, but were not impacted by the Fair Work Act; or the other way around. Flexibility, as measured by agreement with the question "My working times can be flexible", increases by 16 percentage points when a woman becomes mother (off a base of 41%), but this is not systematically different between early and late cohorts. Average number of hours worked from home does not systematically change when women become mothers in the early cohorts, but it noisily increases by half hour among later cohorts (though the difference across cohorts is only significant at the 10% level). This confirms the results discussed in Section 3.

The direct measures of inflexibility do not seem to change systematically, but there is some mild evidence that the Fair Work Act brought with it fewer job switches. In Appendix Table A.13, recall that the top two rows estimate the early-cohorts child penalty in a given variable from moving occupation: for example, in early cohorts mothers experience a drop in "Freedom in decision making" by 0.2 standard deviation simply from moving to an occupation or industry with a lower average value of this variable (in addition to a within-job component, which goes in the same direction). The next two rows estimate the difference in child penalty for late relative to early cohorts. All the coefficients for the differences are positive: given that the baseline coefficients are negative, this indicates fewer job switches. For some variables the estimated difference between early and late cohorts is not significant, but for "Inflexible job" and "Freedom in decision making" it is. This is particularly notable because "Freedom in decision making" is high among more prestigious occupations, such as managers; and this indicates that, after 2009, fewer mothers are switching out from these prestigious occupations.

4.2.2 It is not just a time trend: the discontinuous change in 2009

While the results just presented show that the child penalties along various labor market margins are systematically different between early and late cohorts of mothers, they could just be capturing a time trend. Here we show that, instead, we observe a sharp change, to an otherwise pretty stable pattern of child penalties, precisely around 2009.

Empirical strategy Specifically, we expand on equation (3) by estimating a separate coefficient by cohort:

$$\begin{aligned} Y_{it} = \alpha_i + \delta_t + \beta_{h(i)} + \sum_c \Big\{ & \gamma_{0-5,c} \times \mathbb{1}\{(t - E_i) \in [0,5]\} \times \mathbb{1}\{c = cohort(i)\} + \\ & + \gamma_{6-10,c} \times \mathbb{1}\{(t - E_i) \in [6,10]\} \times \mathbb{1}\{c = cohort(i)\} \Big\} + \epsilon_{it} \end{aligned} \tag{4}$$

where $cohort(i)$ is the cohort i belongs to. In our preferred specification, it corresponds to E_i (year of first childbirth); for certain outcomes we might define cohorts based on pairs of years of childbirth to preserve power. Here, $\gamma_{0-5,c}$ is the 0-5 year child penalty (meaning, the average child penalty in the first five years from the birth of the first child) for mothers belonging to cohort c. The sequence of these coefficients shows us the evolution of the child penalty.

We also estimate a more restrictive model, in which we assume that child penalties follow a linear trend based on the year of first childbirth, but allowing slope and intercept to differ between early and late cohorts. In the graphs, we superimpose it on the coefficient estimates from the previous exercise, as it is helpful to parse trends for some particularly noisy outcomes.

Main outcomes: the evolution of the child penalty in labor supply and earnings Figure 5 shows that the child penalties in labor supply, earnings, and other labor market outcomes are relatively stable across early cohorts and across late cohorts of mothers, but that there was a discontinuous jump in child penalty in 2009, in the direction of greater labor supply and labor earnings. The graphs read as follows: year of first childbirth is on the x-axis, and the corresponding $\gamma_{0-5,c}$, from estimating equation (4), is on the y-axis. The straight lines come from the restricted model.

Earlier in this section we showed that the child penalty in labor force participation, in the first five years after childbirth, is 18% lower for post-2009 relative to pre-2009 first time mothers. Panel (a) of Figure 5 shows that this estimated decrease in the child penalty in labor force participation is not driven by a slow improvement over time, but rather by a sharp change in 2009: if anything, the child penalty in labor force participation was growing *larger* in the years leading up to 2009. For example, the graph shows that for women whose first child was born in 2003, labor force participation drops by 37 percentage points in the first five years after childbirth: this figure is 38 percentage points for women who had their first child in 2005, and 41 pp for women who become mothers in 2008 (39 pp is the pre-2009 average). Instead, women who become mothers in 2010 or 2011 experience a much smaller drop in labor force participation by 30 percentage point, and the figure remains similar for first-time mothers in subsequent years.

The pattern looks even more striking when the outcome is labor supply on the intensive margin (panel (b)): the child penalty in working hours increases in magnitude for the birthing cohorts 2003 to 2008, before discontinuously decreasing after 2009. The figure shows that a woman experiences a drop in hours of work in the five years after childbirth by 16 hours per week if her first child is born in 2003, by 17 if born in 2004 and by 18 if born in 2008 (18 hours/week is the pre-2009 average), but the child penalty in weekly hours of work is 15 hours per week if a woman becomes mother in 2010 or 2012. The sharp change is even more evident in logs (in Appendix Figure A.7). Consistent with the evidence presented above, the child penalty in hourly wages (panel (c)) does not show any systematic pattern, and all confidence intervals include zero: it is therefore unsurprising that the pattern of child penalties in earnings (panel (d)) closely follows that of labor supply.

Other labor market outcomes This exercise is quite revealing also when looking at other outcomes, and helps painting a nuanced picture. Panel (c) in Appendix Figure A.7 shows that the fraction of working women moving to part-time after becoming mothers is increasing in the years between 2003 and 2008, and it starts decreasing after 2009, although not in a sharp manner. This mirrors the hours margin in the first half of the sample, but not in the second half, when also other factors must have been at play. Panel (d) shows a fairly sharp result for what concerns the type of contract: the fraction of working women who move from a permanent position to casual or fixed term employment is fairly stable, around 23 percentage points, for mothers who give birth to their first child between 2003 and 2008, but it drops to about 10 percentage points for mothers who give birth in 2010 and subsequent years.

Persistence In earlier sections we have suggested the hypothesis that a woman's labor supply choices are optimized soon after the birth of her first child, usually within the first year or two, and they tend to be sticky afterwards; here we want to bring some evidence supporting it. First, the event-study graphs shown earlier, in Figure 4, lend support to this hypothesis: for both early and late cohorts, child penalties two or three years after the birth of the first child are very similar to child penalties seven or eight years after childbirth (even though they are quite different between cohorts). Second, in Appendix Figure A.8, we confirm this in a more systematic way. We plot the 0-5 year child penalties in the main labor market outcomes by year of birth of first child (the estimates of the $\gamma_{0-5,c}$'s from equation (4)), as in Figure 5, along with the 6-10 year child penalties (the estimates of the $\gamma_{6-10,c}$'s), meaning the average difference in a given outcome when the first child is between six and ten years old, relative to before childbirth.

The figures support the hypothesis of persistence, pretty strikingly. In panel (a) we see that, although on average the child penalty in labor force participation when the oldest child is between 6 and 10 (in orange) is 8 percentage points lower than when the child is between 0 and 5 (in blue; labor force participation is 8 pp higher when the kid is older), the former very closely tracks the latter, including its ups and down. The child penalty in hours of work conditional on working is extremely persistent: in panel (b), the orange line of estimates of the 6-10 year child penalty perfectly overlaps the blue line of estimates of

the 0-5 year child penalty; and both show a sharp increase in labor supply for post-2009 first births. Panel (c) confirms that also at longer horizons the child penalty in hourly wages is zero.[16] Finally, like in the rest of previous analysis, in panel (d) we see that the child penalty in earnings closely tracks the child penalty in labor supply, in both pattern and persistence.

4.2.3 Robustness checks

In order to formally test for whether the changes we observe in the child penalty happen indeed around 2009, we perform two sets of tests: one is a permutation test based on Ganong and Jäger (2018), and one is a randomization exercise, akin to the one done in Manera and Uccioli (2021).

Permutation test The idea behind this test is simple: re-estimate the basic model "pretending" that the structural break in the time-series of child penalties happened in different years, and compare the true estimates with the placebo estimates; if the true estimate is more extreme than the placebo estimates, you can say with a certain degree of confidence to have found the true structural break.

The 2009 break "passes" the test for most outcome variables, as much as it is possible within this setting. We carry out this permutation test, described in Ganong and Jäger (2018), on all our main outcomes, using as placebo discontinuities all the years in the range 2005 to 2015 (the largest range allowed given the data). Appendix Figure A.9 and Appendix Table A.14 present the results: for the child penalties in labor supply and earnings, the discontinuity between 2009 and 2010 always yields a more extreme estimate than the placebo discontinuities. This is reflected in a p-value of .18 (which is the smallest possible, given the two-sided test and the small number of placebo years). For the child penalty in labor force participation, the 2009-10 discontinuity yields an estimate that is extreme but not the most extreme, with a p-value of .36 in the two-sided test. For the child penalty in hourly wages, for which we don't see anything happening around 2009-10, the estimate at this break sits comfortably in the middle of the distribution of placebo estimates.

Randomization inference In this test, we repeatedly randomly assign mothers to "early" and "late" cohorts and re-estimate the main model, in order to obtain an empirical distribution of t-statistics on the coefficient of interest, against which to compare our baseline estimate. More specifically, at each iteration we randomly assign mothers to a cohort, where a cohort is a fake year of first childbirth (irrespective of the true year of first childbirth), which we use to split them into "early" and "late" cohorts.[17] We then estimate equation (3) and we store the t-statistic for the test of $\gamma_{0-5,post}$ (the difference in child penalty between post-2009 and pre-2009 cohorts of mothers) being equal to zero. We

[16] Moreover, the absence of a large outlier in 2005 for the 6-10 year child penalty reassures that the peak we see for the 0-5 year child penalty is indeed driven by measurement error.

[17] Within mother, the relative-time dummies remain the same: each mother still carries her true child penalty with her, what we randomize is the cohort she belongs to.

repeat this 1,000 times, and then compare the t-statistic computed on the real sample against the randomization estimates, in order to obtain a randomization p-value. The test is described more in detail in Manera and Uccioli (2021).

We perform this test on all of our main outcome variables, and for all of them the t-statistic from the main sample is very extreme relative to the randomization estimates; except for hourly wage. In Appendix Figure A.10 we show the distribution of randomization t-statistics (blue solid line), along with the t-statistic from estimating equation (3) on the main sample (red dashed line), where mothers are assigned to cohorts based on the true year of first childbirth. The distribution of randomization t-statistics is close to a standard normal for all outcomes, while the t-statistic on our baseline estimates is above 3 for all labor supply and earnings outcomes, yielding p-values of zero. Thus, this test strongly rejects the hypothesis that the child penalty in labor supply and earnings for post-2009 cohorts is the same as that for pre-2009 cohorts of mothers. For what concerns the child penalty in hourly wages, instead, the hypothesis of equality between early and late cohorts is not rejected (p-value of 0.19), consistently with the analysis above.

5 Connecting the dots: Exposure to the Fair Work Act via occupation and industry

In order to strengthen the claim that the increase in predictability brought about by the Fair Work Act did cause the change in labor supply, we construct a measure of occupational exposure to the law based on the ex-ante variation in predictability of jobs. We show that the mothers in the most exposed jobs are both those for whom predictability increased the most and those for whom the child penalty in labor supply decreased the most.

5.1 A measure of occupational exposure

The Fair Work Act increased predictability, but it could only do so in jobs where there was scope for a change. We thus develop a measure of exposure of a job to this law based on within-job standard deviation of predictability before 2009, as a proxy for the leeway the provision had in that job. Our definition of “job" is an occupation-by-industry cell.

The intuition behind our exposure measure is the following: jobs in which the schedule was either already fully predictable, or technologically (“naturally") impossible to make predictable, were not affected by the FWA. Recall that we define predictability as working from Monday to Friday on a regular daytime schedule, but the reasoning and the analysis also go through for alternative definitions of predictability, such as not being on call or on an irregular schedule. There are certain jobs that, as of 2009, were already fully predictable, thus a law increasing predictability could have no impact on them. A clerk working in an office only open on weekdays from 9 to 5 would fall into this category. The are other jobs, on the other end of the spectrum, that could not possibly be made predictable: for example, doctors or body guards are often needed at night, during weekends, or in unforeseeable moments. These jobs too would not be impacted by the FWA, which

explicitly states that parents' requests for a change in working arrangements can be refused by an employer on "reasonable business grounds".

The jobs most exposed to the FWA are those that didn't fall into either of these categories, and we capture this through within-job standard deviation of predictability. Intuitively, if, for doing the same job, certain employees have predictable schedules and others do not, we can infer that there are no technological constraints for that job to be made predictable, but that certain employees do not yet enjoy this benefit. As such, there is scope for increasing predictability in the occupation, and such scope is captured by the within-job standard deviation of predictability.

We define "job" as an occupation-by-industry cell, and we construct our proxy of exposure to the FWA using pre-2009 observations. In our definition, "job" is an occupation-by-industry cell, where both occupations and industries are identified at the 2-digit level. In our view, this strikes a good balance between cells that are sufficiently granular that jobs performed are sufficiently similar, while retaining a sufficient sample size for estimating the standard deviation of predictability accurately. We take the value of predictability in all pre-2009 observations for workers in each occupation-by-industry cell, and we compute the within-cell standard deviation: this is our measure of exposure to the law. Our results are robust to using observations only for men for the whole sample period (under the assumptions that men are not treated by the law, both measures should proxy for underlying job "technology" and scope for improvement).

We split the jobs (occupation-by-industry cells) in above and below median exposure; Figure 6 depicts where occupations and industries fall in this classification. Each occupation is present in multiple industries, and each industry contains multiple occupations, which is why each bar includes both red and blue. From the figure, we see that the most exposed jobs are found among office clerks, teaching professionals and other associate professionals, working in education, real estate and R&D, construction. It is reasonable to imagine that office clerks working 9 to 5 jobs coexist with clerks that are required to work on call, and that while some teachers work the same hours every week, others are expected to be available if a colleague is sick; and therefore that, in these occupations and industries, the variation in predictability is high. At the other end of the spectrum, with low within-job variation in predictability, we find jobs in personal and protective services, and in the hospitality industry: most workers in these occupation and industry work outside the standard 9 to 5, Monday to Friday hours, and by their very nature these jobs cannot be made to fit the standard schedule. Hence these jobs have low standard deviation of predictability and low exposure to the law - everybody in these jobs works "unpredictable" hours, and this cannot be changed.

5.2 Empirical strategy

In order to test whether the Fair Work Act did increase predictability, and improved the child penalty in labor supply as a consequence, we assign mothers a level of exposure based on their jobs, and then follow the evolution over time of predictability and of the child penalty of the groups of more and less exposed mothers.

Women are classified as highly or lowly exposed as follows. Each job (occupation-by-

industry cell), as described above, is classified either at high or low exposure according to whether its exposure is above or below median, respectively. Note that this measure is fixed over the sample period for each job. Mothers are then assigned the classification of high or low exposure of the job they were in two years before their first childbirth; for non-mothers (which we keep as controls), we randomly draw fake childbirth years in order to assign the exposure in an analogous fashion. The assigned level of exposure is constant within individuals.

The first analysis we carry out consists in showing the evolution over time of the child penalty, separately for the groups of highly and lowly exposed mothers. In order to do so, we fully interact equation (4) with dummies for being in the high-exposure or in the low-exposure group:

$$\begin{aligned} Y_{it} =& \alpha_i + \sum_{d \in \{\text{high, low}\}} \Bigg\{ \delta_{t,d} + \beta_{h(i),d} + \sum_c \Big\{ \gamma_{0-5,c,d} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{c = cohort(i)\} + \\ &+ \gamma_{6-10,c,d} \times \mathbb{D}_{it}^{6-10} \times \mathbb{1}\{c = cohort(i)\} \Big\} \Bigg\} \times \mathbb{1}\{d = exposure(i)\} + \epsilon_{it} \end{aligned} \tag{5}$$

where $\mathbb{D}_{it}^{0-5} = \mathbb{1}\{(t - E_i) \in [0, 5]\}$ is an indicator function that takes value of one if i's first child is aged 0 to 5 and $\mathbb{D}_{it}^{6-10} = \mathbb{1}\{(t - E_i) \in [6, 10]\}$ is an indicator function for i's first child being between 6 and 10 years old. Here the coefficients of interest are $\gamma_{0-5,c,d}$, which indicate the 0-5 years child penalty in outcome Y for mothers who have had their first child in year c and who belong to exposure group d. For power reasons, in this specification cohorts of mothers are biannual (we group together mothers having their first child in 2003-04, 2005-06, etc.).

The exercise just described recovers the time series of the child penalty for the groups with high and low exposure; here we present the relative difference-in-differences setup. The estimating equation is a little more involved than a standard diff-in-diff because we are not interested in estimating the treatment effect on Y (e.g. labor force participation), but on the pre-post birth *change* in Y (e.g. the child penalty in labor force participation). We formalize it as follows:

$$\begin{aligned} Y_{it} =& +\gamma_a \times \mathbb{D}_{it}^{0-5} + \\ &+\gamma_{a,H} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{exposure(i) = high\} + \\ &+\gamma_b \times \mathbb{D}_{it}^{0-5} \times Post(i) + \\ &+\gamma_{b,H} \times \mathbb{D}_{it}^{0-5} \times Post(i) \times \mathbb{1}\{exposure(i) = high\} + \\ &+\alpha_i + \sum_{d \in \{high, low\}} \{\delta_{t,d} + \beta_{h(i),d}\} \times \mathbb{1}\{d = exposure(i)\} + \\ &+[\ldots] + \epsilon_{it} \end{aligned} \tag{6}$$

where $Post(i)$ indicates that i's first child was born after 2009 (and hence i belongs to the late cohorts, using the terminology from previous sections); and the penultimate line specifies that time and age fixed effects are estimated separately by exposure level. Here γ_a is the baseline 0-5 years child penalty for the low exposure group, and $\gamma_{a,H}$ is the baseline difference in this child penalty for the high exposure group relative to the low exposure

group; these baselines are estimated off the early cohorts (women who became mothers between 2003 and 2008). The coefficient γ_b is the difference in 0-5 years child penalty for the late cohorts relative to the early cohorts in the low exposure group, and $\gamma_{b.H}$ is the difference-in-differences coefficient of interest, which captures the difference in late versus early cohorts of mothers in the high exposure group, relative to the same difference for the low exposure group.

5.3 Results

The entire change in predictability and the entire improvement in intensive labor supply are concentrated in the group of mothers in more-exposed occupations, supporting the hypothesis that the increase in predictability brought about by the Fair Work Act is responsible for the improvement in maternal labor supply (decrease in child penalty).

Figure 7 presents the time evolution of the child penalties separately by exposure – the results of the first piece of analysis. This figure basically replicates Figure 5 but separately for the high and low exposure groups. More formally, it displays the estimated $\gamma_{0-5,c,d}$'s from equation (5): the dashed blue line plots the evolution over time of the 0-5 years child penalty for mothers in the low exposed group (jobs with below-median standard deviation of predictability), and the solid red line does the same for the high exposed group (above-median). Note that the result for predictability is also cast in terms of child penalty for easier comparison: panel (d) in Figure 7 does not display average predictability by cohort, but average *drop* in predictability after childbirth relative to before, by cohort. (As an aside, the *pre-birth levels* of predictability are fairly constant across the cohorts, hence the patterns shown for the child penalty are fully driven by *post-birth* variations in predictability, which strengthens our interpretation. Note that this statement is true for all the outcome variables we consider. The graphs showing raw averages of variables of interest, pre and post birth, by cohort, are available upon request).

The child penalties in predictability and intensive-margin labor supply are fairly constant over the sample period for the low-exposure group, while they display sharp changes around 2009 - in the direction of greater predictability and greater labor supply - for the high exposure group. Panel (d) of Figure 7 confirms that indeed the child penalty in predictability changed sharply after 2009, but only for mothers working in the jobs most exposed to the law: the jobs less exposed to the law show no change in the child penalty in predictability over the sample period, reassuringly. Panels (b) and (c) display the evolution of child penalty in labor supply on the intensive margin, for weekly hours in paid employment and log weekly hours in paid employment respectively. In both panels we see that the blue dashed line, plotting the coefficients for the low exposure group, is fairly flat over the sample period; while the red solid line shows a sharp and permanent increase in the intensive margin labor supply (a decrease in the child penalty) after 2009, with a timing that tracks almost perfectly the changes in predictability. Finally, panel (a) shows that this heterogeneity does not explain the changes in the child penalty in extensive margin labor supply, as the two exposure groups closely track each other along this dimension.

In Table 5 we confirm the significance, or lack thereof, of the patterns just described. In the even-numbered columns of this table we report the coefficient estimates from equa-

tion (5): the first row reports the estimates of γ_a (baseline child penalty for low exposure group), the second those of $\gamma_{a,H}$ (baseline difference in child penalty between the high and low exposure group), the third those of γ_b (late-early cohort difference for the low exposure group) and the fourth those of $\gamma_{b,H}$ (difference-in-difference coefficient, our coefficient of interest); the odd-numbered columns report the relative coefficient from a basic specification that doesn't distinguish between exposure groups.

The child penalty in predictability (columns 7 and 8) changed systematically for late cohorts of mothers relative to early cohorts of mothers for the highly-exposed mothers; relative to the lowly-exposed mothers, for whom it didn't change at all. Column 8 tells us that at baseline, women in the low exposure group are 33 percentage points less likely to have a regular schedule after they become mothers relative to before, but this figure is almost twice as large for women in the high exposure group. It also tells us that there is virtually no difference in the child penalty in predictability between late and early cohorts in the low exposure group, but, crucially, the difference-in-differences coefficient indicates that the change for the high exposure group is very large (19 percentage points) and highly significant. This confirms that the passage of the FWA did have a large impact on the predictability of mothers of young children in the high exposure group, and none in the low exposure group.

Correspondingly, the child penalty in intensive margin labor supply shrank dramatically in the late cohorts of mothers relative to the early cohorts for the highly-exposed group; relative to the lowly-exposed group where it basically did not change. Column 4 in Table 5 shows that at baseline women in low-exposed jobs suffer a child penalty of 16 hours of work per week, and women in high exposed jobs of about 20 hours a week. Among women in the late cohorts (giving birth after 2009), those in low exposure jobs suffer a very similar child penalty as early cohort (their post-birth labor supply is about 0.8 hours higher, but not significantly different), but those in high exposure job experience an increase in post-birth labor supply (a decrease in the child penalty) of about 5.5 hours a week, relative to the late-early comparison for the low exposure group. In summary, women in the high exposure group after the passage of the law experience a child penalty in intensive margin labor supply that is about 30% smaller than before the FWA, while for the low exposure group this difference is 5% and not statistically different from zero.

These effects on the child penalty in hours of work are driven by fewer women switching to part-time jobs in the high-exposure group, and also hold in the longer term. Appendix Table A.16 shows that, in the high exposure group, the share of women moving to part-time after they become mothers is 20 percentage point smaller for late cohorts relative to early cohorts, relative to the low exposure group. While Table 5 only presents the coefficients on the 0-5 year child penalty, meaning the child penalty estimated on the first five years from the birth of the first child (for clarity of exposition), Appendix Table A.15 extends it to include coefficients for the 6-10 year child penalty, meaning the child penalty estimated in the 6 to 10 years after the birth of the first child (relative to before childbirth, and to non-mothers). In columns 2 and 4, we can see that both the estimated increase in predictability and the estimated increase in intensive-margin labor supply (decrease in child penalty) for more exposed mothers still hold when we look at the longer term, although more noisily.

Finally, exposure to the FWA as we measure it does not explain the observed change

in the child penalty in labor force participation. Column 1 of Table 5 summarizes what we have shown in Section 4.2.2: that there is a change in the child penalty in labor force participation between early and late cohorts of mothers (although statistical significance for this variable is less clear-cut than for intensive-margin labor supply, as we discuss in that section). Column 2, tough, shows that high and low exposure groups have very similar child penalties in labor force participation both at baseline and after the passage of the law: thus, exposure does not explain the early-late cohort difference in child penalty in labor supply we observe.

To conclude: we presented the results using the above/below median exposure split for clarity of exposition, but they also hold if we use the measure of exposure directly. We visualize this in Appendix Figure A.12, where we plot on the x-axis our measure of exposure (the standard deviation of predictability), and on the y-axis the reduction in child penalty in hours of work between late and early cohorts (higher numbers mean higher post-childbirth labor supply – hence smaller child penalty – in late cohort relative to early cohorts). Both are computed at the level of occupation-by-industry and "binscattered" here. We see a fairly linear relationship between the two variables: for low values of exposure, the change in the child penalty between early and late cohorts is essentially zero; for middle levels of exposure, late cohorts experience a child penalty in labor supply which is up to 5 hours smaller than early cohorts; at high level of exposure, the difference in child penalty between late and early cohorts can be up to 10 hours a week. A version of Table 5, where we replace the above/below median split with a linear interaction with exposure, confirms the linearity in this relationship and yields the same message as Table 5; it is not shown here but available upon request.

6 Robustness check: Paid Parental Leave is not driving the results

We have shown in the previous section that the large drop in child penalty in labor supply that occurs around 2009 is entirely driven by the increase in workplace predictability. Here we rule out an important potential confounder: the 2010 introduction of Paid Parental Leave (PPL). We show that our results are unaffected when we account for Paid Parental Leave and that Paid Parental Leave *per se* does not seem to have significantly affected the child penalty in labor supply, a result in line with previous literature, and which is not surprising given the pre-existing institutional context.

6.1 Institutional setting

Paid Parental Leave was introduced a year after the Fair Work Act, and could thus act as a confounder. However, the pre-existing presence of unpaid leave with job guarantee for up to one year, and a cash transfer for new babies, implies that this new piece of legislation did not substantially change incentives for new mothers.

Paid parental leave was signed into law in 2010 (and applies since January 1, 2011), and it allows one of the parents to take up to 18 weeks off from work, paid at the national

minimum wage, after the birth of a child. PPL could not be combined with the Baby Bonus, an unconditional and tax-exempt cash transfer that was available to all families in which new babies were born. While PPL is nominally more generous, the interaction with the tax and transfer system (including Family Tax Benefits, a set of transfers specifically for lower income families) meant that for a fraction of families it was less advantageous than the already existing Baby Bonus, and for another set the two were close substitutes. The remaining group, relatively wealthier mothers, were facing a relatively lower replacement rate (since PPL is paid at the minimum wage regardless of pre-birth earnings) and were more likely to be eligible for employer-provided paid maternity leave, thus it is unclear whether this policy changed their choice set either.

In addition, paid leave guarantees job protection – and this is the aspect that really matters for labor force attachment of new mothers, according to the review article by Olivetti and Petrongolo (2017). However, up to one year of unpaid job-protected leave upon the birth of a child was already granted to all Australian workers since 1979, so the introduction of PPL didn't change this aspect either. In conclusion, it seems that the introduction of PPL did not significantly change the incentives faced by new parents, and therefore we do not expect it to lead to significant changes in their labor supply choices. For a more detailed explanation of family benefits in Australia, and how they have changed over the past twenty years, see Bassford and Fisher (2016); Kalb (2018); de Gendre et al. (2021).

6.2 Empirical strategy

In order to study whether Paid Parental Leave affected the child penalty in labor supply, we leverage the fact that before the introduction of PPL, 55% of working women were already eligible for employer-provided paid maternity leave. Under the assumption that the introduction of national, state-funded, Paid Parental Leave does not affect women who already can access this benefit through their employer, the evolution of the child penalty for non-eligible women relative to that of eligible ones estimates the effect of introducing paid parental leave.

First, we study the effect of PPL on maternal labor supply by comparing the evolution of the child penalty for treated and non-treated women. We define women as being treated by PPL if, two years before the birth of their first child, they were *not* eligible for employer-provided PPL; for non-mothers (which we keep as controls) we randomly draw fake childbirth years to assign treatment analogously. Then, we run a specification analogous to equation (6), where we replace the dummy of high exposure to the FWA with a dummy for being treated by PPL.

Second, we test whether PPL might be the omitted variable driving our results from the previous sections. If the jobs that offer employer-provided paid leave are systematically more or less likely to be exposed to the changes of the Fair Work Act, we might attribute the change in the child penalty to the FWA when in fact the observed effect is driven by the correlation between job predictability and likelihood of being offered paid maternity leave. We address this issue by augmenting the specification above to include both controls for

high-exposure to FWA and for being treated by PPL, as follows:

$$
\begin{aligned}
Y_{it} = & +\gamma_a \times \mathbb{D}_{it}^{0-5} + \\
& +\gamma_{a,H} \times \mathbb{D}_{it}^{0-5} \times \mathbb{1}\{exposure(i) = high\} + \\
& +\gamma_{a,nP} \times \mathbb{D}_{it}^{0-5} \times \mathbb{nPPL} + \\
& +\gamma_b \times \mathbb{D}_{it}^{0-5} \times Post(i) + \\
& +\gamma_{b,H} \times \mathbb{D}_{it}^{0-5} \times Post(i) \times \mathbb{1}\{exposure(i) = high\} + \\
& +\gamma_{b,nP} \times \mathbb{D}_{it}^{0-5} \times Post(i) \times \mathbb{nPPL} + \\
& +\alpha_i + \sum_{p \in \{yes,no\}} \sum_{d \in \{high,low\}} \{\delta_{t,d,p} + \beta_{h(i),d,p}\} \\
& \quad \times \mathbb{1}\{d = exposure(i)\} \times \mathbb{1}\{p = PPL - treated(i)\} + \\
& +[\ldots] + \epsilon_{it}
\end{aligned}
\tag{7}
$$

where $\mathbb{1}\{exposure(i) = high\}$ is a dummy indicating high exposure to the Fair Work Act, as defined in Section 5, and $\mathbb{nPPL}$ indicates that individual i was *not* offered paid maternity leave by her employer two years before childbirth, and hence is treated by the law introducing paid parental leave. The last couple of lines specify that time and age fixed effects are estimated separately for the four groups defined by the interaction of the two classifications. In this specification, the coefficient $\gamma_{b,nP}$ is the effect of PPL on the child penalty in Y, controlling for exposure to the FWA, and $\gamma_{b,H}$ is the effect of exposure to the FWA on the child penalty in Y, controlling for PPL.

6.3 Results

By leveraging access to employer-provided paid leave to proxy for treatment by PPL, as described above, here we show that we cannot reject the null hypothesis that PPL did not have an effect on maternal labor supply, and that the main results of the paper presented in the section above are robust to controlling for PPL.

PPL does not have a statistically significant impact on the child penalty in labor supply, although the point estimates suggest that it might have helped increase labor force participation of new mothers. From column 3 in Table 6 we learn that, among mothers whose first child was born before 2009, those not eligible for employer-provided paid parental leave were 6 percentage points less likely to return to work after childbirth, and the difference between the two groups disappears among mothers whose first child was born after 2010; however, neither the "diff" nor the "diff-in-diff" coefficients are significantly different from zero. The child penalty in log hours in paid employment (column 7) is basically identical for mothers who were and were not eligible for employer-provided paid leave throughout the sample period. When we look at raw hours (column 3 of Appendix Table A.17), we estimate a marginally significant negative effect of PPL on labor supply, but this comes from the correlation between being eligible to employer provided paid parental leave and exposure to the FWA, as the coefficient becomes insignificant when we control for exposure to the FWA – to which we turn next.

Controlling for PPL does not change the results based on exposure to the FWA presented in Section 5. In column 2 [6] of Table 6 we replicate the analysis from Section 5 on the

sample in which we can observe eligibility to employer-provided paid parental leave (and hence assign PPL-treatment); on column 4 [8] we report the coefficients from estimating equation (7): the only difference between the two is the inclusion of controls for PPL-treatment status in the latter. In columns 2 and 4 the outcome variable is labor force participation, while in columns 6 and 8 it is the log of weekly hours in paid employment. In both instances, the inclusion of the controls for PPL does not change the coefficients on being exposed to the FWA. This confirms that the results in Section 5, that attribute the decrease in child penalty to exposure to the FWA, were not driven by the contemporaneous passage of Paid Parenal Leave.

7 Future work: child penalty in housework and intra-household time allocation

On top of exploring the child penalty in labor market outcomes, this setting and data allow us to study intra-household decision making when children are born. The key ingredients are the following: we document that at baseline women dramatically reallocate time use when they become mothers, while fathers don't; we show that the maternal labor supply increase following the FWA comes at the expenses of housework but not of time spent playing with children, and that fathers only pick up a small portion of the slack in housework. In future work, we will leverage these key ingredients to speak to the determinants of intra-household allocation of tasks around parenthood and shed further light on the causes of the child penalty.

7.1 Child penalty in time use

We have shown that having a child has dramatic effects on the labor supply of a mother, substantially reducing the time she spends at work. Here we show that she reduces time spent in all other activities, except for housework which doubles, in order to spend more than 50 hours a week playing with the newborn. A father does not change very much time spent at work or doing housework after the birth of his first child, but he spends about 18 hours a week playing with his newborn.

Having a child is a major event in the allocation of a woman's time, while on many dimensions it is a non-event for fathers. Figure 8 shows time allocation in the years relative to the birth of the first child, separately for women (panel (a)) and for men (panel (b)). Five years before the birth of her fist child, a woman spends on average 32 hours a week in paid employment (in red), 7 hours a week doing housework (in green), and about 7 hours a week in physical and social activities. The missing category includes other types of leisure and, most notably, sleep. Five years before the birth of his first child, a man allocates his time in a similar manner: he spends 38 hours a week in paid employment, 5 hours a week in housework, and 7 hours a week in physical and social activities. As the birth of the first child nears, time use is mostly unchanged for both men and women, except for a gradual increase in the time a woman spends doing housework – presumably corresponding to the increased demands for housework from cohabitation.

The birth of the first child brings with it a dramatic change in the time use for women. We visualize it in panel (a) of Figure 8; the corresponding coefficients from the child penalty regression are in panel A of Table 7. The red area of the graph, corresponding to paid employment, shrinks dramatically (unconditional hours of work drop on average 24 hours a week in the first two years from the birth of the child), as we have discussed in previous sections. The white area of the graph, which is everything that it is left unaccounted for – for the greatest part, leisure and sleep – also shrinks drastically. This makes space not only for time spent playing with the child (50 hours a week in the first two years from childbirth, in blue), but also for housework (in green), which more than doubles, from 9 to 18 hours a week on average. Even ten years after becoming mothers, when they "only" spend roughly 30 hours a week playing with their children, women still spend 20 hours a week in housework, and only marginally increase time spent in paid employment.

Men's time use is mostly unchanged when they become fathers, except that they do spend some time playing with their child. This is visually clear in panel (b) of Figure 8; we also report coefficient estimates in Appendix Table A.19. Time spent in employment, roughly 40 hours a week before the child is born, decreases by roughly two hours a week after they become fathers; time spent doing housework increases from 5 to 6 hours a week over the first ten years after the birth of their first child. In the first few years after becoming fathers, men spend an average of 18 hours per week playing with the child, at the expense of the residual category – leisure and sleep.

From the comparison we learn that, while before becoming parents, women work a bit less than men (32 versus 38 hours/week) and take care of the house slightly more (7 versus 5 hours), these differences are greatly magnified by parenthood, when women work around 40% of the hours men do, while carrying out three times as much housework and spending three times as much time playing with the child.

7.2 Ramifications of the Fair Work Act: change in time use

The Fair Work Act was followed by an increase in maternal labor supply, which came at the expense of housework but not of time playing with children, suggesting a hierarchical perception of tasks where playing with a child is prioritized over having a clean house. Fathers only partially pick up the slack in housework.

We compute the child penalty in non-market work and study how it changed around 2009, in the same ways as we did for the child penalty in labor market outcomes in Section 4.2. We start with the early versus late cohorts comparisons: we estimate equation (2) on the number of hours per week spent in housework tasks and on the weekly hours spent playing with children. We do this separately for early cohorts of mother (first childbirth in 2005-2008) and late cohorts of mothers (2010-2013). Panels (a) and (b) of Figure 9 display the estimated γ_k's for housework and playing with kids, respectively; the green circles are the estimates for the early cohorts and the red squares for the late cohorts.

For all mothers the time spent in domestic chores increases substantially after the birth of the first child, but for later cohorts of mothers it increases by less, mirroring the results for time in paid employment. In panel (a) we observe that, while the time spent in housework increases by about 8 hours per week on the year of birth for both groups, for early

cohorts this figure goes significantly up in the following years (reaching a peak of 13 hours per week) but the corresponding increase for later cohorts is more modest (only peaks at about 10 hours per week). This difference of approximately 3 hours per week is analogous to the difference between the two groups in weekly hours worked, hence suggesting that after 2009 women trade, at least in part, hours worked in domestic chores for hours worked in the labor market.

Time spent directly with children, instead, is not affected by the increase in hours in paid employment. From panel (b) we can see that there is no difference in the two groups when the outcome is time spent playing with children: on the year of birth this figure increases by 51 hours for both cohorts, and in the subsequent years both figures steadily decline, so that 9 years after childbirth the penalty is 31 for the early cohorts and 34 for the late cohorts (not statistically different). This suggests that, even if working more, women don't renounce to spending time with their children.

7.3 Future Work

The patterns described above suggest an unequal split of the burden of a child between the parents, which is not consistent with a rational reaction to labor market opportunities, given that changing opportunities do not translate into a change in tasks allocation. In future work, we plan to model these patterns to speak to intra-household bargaining in the presence of children.

We have shown that the relaxation of a constraint in working arrangements, and the increase in scheduling predictability that follows, lead to a relative increase in labor supply of new mothers, but we yet don't know the mechanisms behind this choice. We posit that we need two sets of results in order to sensibly formalize the mapping between working arrangements and maternal labor supply, and write a model that could inform policy-making. The first relates to time – what is the extra time devoted to paid work taken out from? The second relates to partners – it (generally) takes two people to have a baby: how is the time burden that come with children split between the two parents? Does it change with changing work prospects? Is it the outcome of a bargaining process, or is it a mother's individual decision?

In this section we have made progress on answering the first question: we have shown that the increase in labor supply comes at the expense of time spent in house chores but not time spent with children. In future work we plan to address the second question, by running the analysis separately for partnered and unpartnered mothers, and by studying whether men's labor supply and housework are affected by their partner's adjustments of hours.

8 Conclusion

In this paper we have studied the role of working arrangements in the child penalty in labor supply, the large drop in labor supply women face when becoming mothers. By leveraging the introduction of the 2009 Australian Fair Work Act, which entitled parents of young children to a change in working arrangements, we established two sets of facts. First,

when allowed to request not better defined "changes in working arrangements", mothers of young children choose to increase the predictability of their schedules, rather than their flexibility. Second, an increase in predictability is associated with a smaller child penalty on the intensive margin labor supply: when their schedules are more predictable, mothers of young children work longer hours.

We have provided further support for the hypothesis that the observed changes in the child penalty were caused by the changes in predictability brought by the Fair Work Act in two ways. First, we used a measure of exposure to the Fair Work Act, based on a proxy for how much scope for improvement there was in a given occupation-by-industry, to show that the entire increase in predictability and the full decrease in the child penalty in labor supply are concentrated in the group of women who were most exposed to the law through their pre-motherhood job. Second, we have provided evidence that our results are not driven by the contemporaneous introduction of Paid Parental Leave.

We conclude the paper opening the way for further research in this area. First, our paper shows the existence of a "child penalty" in working arrangements, and that when these working arrangements are allowed to change, labor supply changes as well. Further research is warranted to explore further both which working arrangements are predictive of the size of the child penalty, and what we can learn from the way working arrangements change around childbirth. Second, changing labor supply around childbirth is part of an overall rearrangement of tasks within the household. Studying how time is reallocated within person, and how tasks are allocated across partners in a couple, is going to help shed more light on the root causes of the child penalty in labor market. This is where we are going next.

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9 Figures

Figure 1: “Regular schedule” for mothers relative to non-mothers, by year and age of child

Note: This figure represents the evolution over time of predictability for women over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is “Regular Schedule", a dummy for working from Monday to Friday on a regular daytime schedule. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimes for different ranges of child’s age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure 2: Fertility did not change discontinuously around 2009

Note: This figure plots the fertility rate (for first births) in our sample over the sample period. Each dot represents the total number of women giving birth to their first child in the corresponding year divided by the total number of women in our sample in that year. The vertical dotted line indicates the year 2009, in which the Fair Work Act was introduced.

Figure 3: Child penalty by gender: main labor market outcomes

(a) Labor force participation

(b) Weekly hours in paid employment (conditional on working)

(c) Hourly wage

(d) Log of (1 + weekly wages)

Note: This figure shows the child penalty in labor supply, wages, and earnings by year relative to childbirth for both men and women, as described in Section 4.1.1. Namely, the figure plots the coefficients γ_k's from estimating equation (2), which includes individual, time and age fixed effects. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. In all panels, the blue squares are estimates from a regression that includes only men, while the pink circles are estimates from a regression that includes only women. The shades around them are 95% confidence intervals. We only include parents who have their first child between 2003 and 2008, and we include non-parents as controls - see Section 4.1.1 for details. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level.

Figure 4: Change in the child penalty for women: Early versus Late cohorts (pre vs post 2009)

(a) Labor force participation

(b) Weekly hours in paid employment (conditional on working)

(c) Hourly wage

(d) Log of (1 + weekly wages)

Note: This figure shows how the child penalty in labor supply, wages, and earnings for women has changed over the sample period. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. All panels plot the child penalty in labor supply by year relative to childbirth (the coefficients γ_k's in equation (2)) for women, estimated separately for early cohorts (first childbirth between 2005 and 2008, green circles) and late cohorts (first childbirth between 2010 and 2013, red squares). Women without children are included in both regressions as controls, and women having children in other sample years are excluded from both regression. See note to Figure 3 for further details.

Figure 5: Time evolution of the child penalty for women

(a) Labor force participation

(b) Weekly hours in paid employment (conditional on working)

(c) Hourly wage

(d) Log of (1 + weekly wages)

Note: This figure shows how the child penalty in labor supply, wages, and earnings for women has changed over the sample period. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. All panels display the 0-5 years child penalty (average of the child penalty from zero to five years after the birth of the first child) for women by year of birth of the first child. That is, they plot the estimates of $\gamma_{0-5,c}$ from equation (4). The blue circles are the estimates yearly cohort-by-yearly cohort, and the shades around them are 95% confidence intervals. The solid lines are the linear trends estimated for childbirth cohorts 2003-2008 and 2010-2017, and the dashed lines are the corresponding 95% confidence intervals, as described in Section 4.2.2. We cluster standard errors at the individual level.

Figure 6: Exposure to the Fair Work Act: Standard deviation of predictability of occupations and industries

(a) Occupations

(b) Industries

Note: For each occupation (left panel) and industry (right panel) in our sample, this figure plots the frequency of observations below and above the median standard deviation of predictability. Since predictability is defined at the industry-by-occupation level, each industry and each occupation can include jobs with different values of flexibility. Jobs (occupation-by-industry cells) are weighted by the number of pre-2009 observations, including both women and men.

Figure 7: Time evolution of child penalty by job predictability

(a) Labor force participation

(b) Weekly hours in paid work (conditional on employment)

(c) Log weekly hours in paid work

(d) Regular schedule

Note: This figure shows the evolution of the child penalty in labor supply and predictability for women over the sample period, separately by levels of exposure the Fair Work Act, as described in Section 5. Specifically, exposure of a job is defined as the standard deviation in predictability using pre-2009 observations; the exposure of a woman is that of the job (occupation-by-industry) she had two years before childbirth. The blue dotted lines are the estimates for the low-exposure group, women in jobs with below-median standard deviation of predictability; the red solid lines plot the same estimates for the high exposure groups, women in jobs with above-median standard deviation of predictability. More precisely, dots represent the estimates for $\gamma_{0-5,c,d}$ from equation (5). Cohorts are biannual (i.e. we pull together women whose birth was in 2003 and in 2004, in 2005 and in 2006, etc; except for 2009, which is kept separate). The vertical segments represent 95% confidence intervals. Women without children are included as controls, and exposure is assigned based on the job they were in two years before a randomly-drawn fake childbirth year. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is the natural logarithm of weekly hours in paid employment; in panel (d) the outcome is the regular schedule dummy (equal to 1 if the respondent has a Monday to Friday schedule and a regular daytime schedule). We cluster standard errors at the individual level.

Figure 8: Time use around the birth of the first child by gender

(a) Women (b) Men

Note: This figure represents the average number of weekly hours spent in different activities, for women (panel (a)) and men (panel (b)) by time relative to the birth of their first child. For both panels, the horizontal axis represents years to and from childbirth, and the vertical axis represents weekly hours. A description of how the times use variables are constructed can be found in Appendix B.

Figure 9: Time evolution of child penalty in non-market work

(a) Weekly hours in housework, pre and post 2009 (b) Weekly hours playing with kids, pre and post 2009

Note: This figure shows how the child penalty in non-market work has changed for women over the sample period. In panel (a) the outcome is the number of weekly hours devoted to housework; in panel (b) the outcome is the number of weekly hours spent playing with children. Both panels display the child penalty by year relative to childbirth for women (the coefficients γ_k's in equation (2)), estimated separately for early cohorts (first childbirth between 2005 and 2008, green circles) and late cohorts (first childbirth between 2010 and 2013, red squares). Women without children are included in both regressions as controls, and women having children in other sample years are excluded from both regression.

10 Tables

Table 1: Summary Statistics

	N	Mean	SD	Min	Max
Women					
Age	132391	38.8	14.3	15	65
In labor force	126624	0.72	0.45	0	1
Weekly hours in paid employment (conditional)	85707	31.4	14.2	1	144
Log weekly hours in paid employment	85707	3.30	0.64	0	4.97
Log(1+weekly wage)	132391	4.09	3.18	0	8.36
Log(1+annual labor income)	132391	7.11	4.75	0	12.3
Hourly wage	85707	25.2	27.1	0	2000
Log hourly wage	79681	3.14	0.55	-3.00	7.60
Part-time if employed	85790	0.48	0.50	0	1
Permanent emplyment if employed (vs casual and fixed-term)	76564	0.63	0.48	0	1
Overtime (more than 38 hours/week)	132391	0.19	0.39	0	1
Overtime (more than 50 hours/week)	132391	0.053	0.22	0	1
Promoted in the past year (conditional on current employment)	74091	0.091	0.29	0	1
Normally supervise work of other employees	85906	0.41	0.49	0	1
Occupational status scale	85876	52.3	22.6	0	100
Regular schedule (M-F and regular daytime schedule)	85880	0.42	0.49	0	1
M-F	85885	0.45	0.50	0	1
Regular daytime schedule	85906	0.75	0.44	0	1
On call and irregular shifts	85906	0.10	0.30	0	1
My working times can be flexible (yes-no)	63040	0.49	0.50	0	1
Entitled to flexible start/finish times	67070	0.56	0.50	0	1
Has bachelor degree	126588	0.27	0.44	0	1
Men					
Age	125761	38.5	14.4	15	65
In labor force	116100	0.84	0.36	0	1
Weekly hours in paid employment (conditional)	91809	41.7	14.4	1	150
Log weekly hours in paid employment	91809	3.64	0.51	0	5.01
Log(1+weekly wage)	125761	4.87	3.19	0	8.84
Log(1+annual labor income)	125761	8.21	4.56	0	12.9
Hourly wage	91809	26.8	22.9	0	1611
Log hourly wage	81768	3.24	0.59	-3.69	7.38
Part-time if employed	91890	0.17	0.38	0	1
Permanent emplyment if employed (vs casual and fixed-term)	74868	0.72	0.45	0	1
Overtime (more than 38 hours/week)	125761	0.47	0.50	0	1
Overtime (more than 50 hours/week)	125761	0.19	0.39	0	1
Promoted in the past year (conditional on current employment)	76366	0.098	0.30	0	1
Normally supervise work of other employees	91975	0.50	0.50	0	1
Occupational status scale	91926	47.2	23.7	0	100
Regular schedule (M-F and regular daytime schedule)	91966	0.53	0.50	0	1
M-F	91978	0.57	0.49	0	1
Regular daytime schedule	91981	0.75	0.43	0	1
On call and irregular shifts	91981	0.10	0.30	0	1
My working times can be flexible (yes-no)	64191	0.52	0.50	0	1
Entitled to flexible start/finish times	68387	0.60	0.49	0	1
Has bachelor degree	116056	0.22	0.41	0	1

Note: The table shows summary statistics for the full sample from HILDA, separately by gender. The dataset covers the years 2001-2019. We restrict the sample to people aged 15 to 65, and exclude individuals who have their first child in the first and last two years of the sample (2001-02 and 2018-19). For the individuals who have their first child in the years 2003-2017, we exclude observations earlier than six years prior to the childbirth and later than ten years afterwards. All variables are defined in Appendix B.

Table 2: Balance Table: Early vs Late Cohorts of Mothers-to-be

	Early cohort (2003-2008)			Late cohort (2010-2017)				
	N	Mean	SD	N	Mean	SD	Diff.	SE
Year first birth	434	2005.54	(1.73)	800	2013.80	(2.20)	8.25***	(0.12)
Age at first birth	434	28.74	(6.09)	800	29.04	(5.50)	0.30	(0.34)
In labor force	428	0.93	(0.26)	797	0.93	(0.25)	0.00	(0.02)
Weekly hours in paid employment (conditional)	375	39.19	(10.86)	700	38.17	(10.61)	-1.03	(0.68)
Log weekly hours in paid employment	375	3.61	(0.40)	700	3.58	(0.42)	-0.03	(0.03)
Log(1+weekly wage)	434	5.46	(2.41)	800	5.74	(2.52)	0.29	(0.15)
Log(1+annual labor income)	434	8.78	(3.58)	800	9.68	(3.08)	0.90***	(0.19)
Hourly wage	375	18.39	(8.52)	700	25.90	(11.80)	7.51***	(0.69)
Log hourly wage	367	2.84	(0.46)	675	3.21	(0.40)	0.38***	(0.03)
Part-time if employed	375	0.21	(0.40)	700	0.20	(0.40)	-0.00	(0.03)
Permanent emplyment if employed (vs casual and fixed-term)	353	0.71	(0.45)	668	0.75	(0.43)	0.03	(0.03)
Overtime (more than 38 hours/week)	434	0.47	(0.50)	800	0.43	(0.50)	-0.04	(0.03)
Overtime (more than 50 hours/week)	434	0.12	(0.33)	800	0.09	(0.29)	-0.03	(0.02)
Promoted in the past year (conditional on current employment)	275	0.18	(0.38)	612	0.18	(0.38)	0.00	(0.03)
Normally supervise work of other employees	375	0.51	(0.50)	700	0.47	(0.50)	-0.04	(0.03)
Occupational status scale	375	54.72	(23.14)	699	57.91	(22.53)	3.19*	(1.46)
Regular schedule (M-F and regular daytime schedule)	375	0.59	(0.49)	700	0.63	(0.48)	0.04	(0.03)
M-F	375	0.62	(0.49)	700	0.65	(0.48)	0.02	(0.03)
Regular daytime schedule	375	0.80	(0.40)	700	0.83	(0.38)	0.03	(0.02)
On call and irregular shifts	375	0.06	(0.23)	700	0.05	(0.22)	-0.01	(0.01)
My working times can be flexible (yes-no)	110	0.37	(0.49)	609	0.45	(0.50)	0.07	(0.05)
Entitled to flexible start/finish times	243	0.53	(0.50)	555	0.59	(0.49)	0.06	(0.04)
Has bachelor degree	428	0.38	(0.49)	797	0.39	(0.49)	0.01	(0.03)

Note: The table shows the summary statistics for the sample of will-be mothers. All variables except for age are measured two years before childbirth. Early cohorts include women who had their first childbirth between 2003 and 2008, and late cohorts include women whose first child was born between 2009 and 2017. We restrict the sample to people aged 15 to 65. For the descriptions of the variable see Appendix B. Stars indicate statistical significance (+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$).

Table 3: Changes in working arrangements for mothers

	Predictability				Flexibility		Work from home			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Regular schedule (M-F and regular daytime schedule)	M-F	Regular daytime schedule	On call and irregular shifts	My working times can be flexible (yes-no)	Entitled to flexible start/finish times	Hours work from home	Any hours worked from home	Hours work from home conditional on any	Entitled to home-based work
First child 0-2	0.104***	0.0975***	0.0768***	-0.0288+	-0.0615*	0.00500	0.508+	-0.0379+	2.900**	-0.0268
× First birth after 2010	(0.0235)	(0.0239)	(0.0200)	(0.0157)	(0.0303)	(0.0236)	(0.294)	(0.0221)	(1.024)	(0.0232)
First child 3-5	0.0578*	0.0633*	0.0737***	-0.0450**	-0.0106	0.0205	0.880*	0.0319	1.779+	0.0161
× First birth after 2013	(0.0266)	(0.0267)	(0.0217)	(0.0170)	(0.0282)	(0.0256)	(0.387)	(0.0230)	(0.989)	(0.0250)
First child 6-8	0.0906**	0.0757*	0.0708**	-0.0265	-0.0120	0.0305	1.248**	0.0563*	1.831	0.0156
× First birth after 2016	(0.0310)	(0.0317)	(0.0230)	(0.0173)	(0.0299)	(0.0266)	(0.440)	(0.0254)	(1.155)	(0.0252)
Pre-period mean:										
First child aged 0-2	0.266	0.294	0.753	0.102	0.587	0.636	2.574	0.313	8.372	0.353
First child aged 3-5	0.282	0.300	0.759	0.123	0.579	0.638	2.823	0.310	9.265	0.381
First child aged 6-8	0.299	0.333	0.784	0.0902	0.556	0.617	3.140	0.329	9.647	0.329
Observations:										
N	83526	83532	83553	83553	61095	64853	88671	88671	17679	60302
N Individuals	9780	9781	9781	9781	8257	8489	10530	10530	3116	8244

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows the estimates for the coefficients of a restricted version of equation (1), where we only include the dummies based on the age of the first child (meaning, we drop all the dummies based on the age of the youngest child), and we replace the year-by-year interactions with mobile "post" dummies. In particular, we interact the dummy for the first child being between 0 and 2 with an indicator for the year being equal or after 2010; the dummy for the first child being between 3 and 5 with an indicator for the year being equal or after 2013; the dummy for the first child being between 6 and 8 with an indicator for the year being equal or after 2016. These coefficients are reported in the first three lines. The regressions also include the dummies based on the age of the first child (without interactions) for age groups 0-2, 3-5, 6-8, 9-10, more than 10; as well as individual, time and age fixed effects; these coefficients are not reported. The bottom panel includes the pre-period means of the outcome variables, namely averages for the relevant group of the years for which the mobile "post" dummy is zero. Standard errors are clustered at the individual level. The outcomes considered are measures of predictability (for columns 1-4), flexibility (for columns 5 and 6), and work from home (for columns 7-10). All variables are defined in Appendix B.

Table 4: Child penalties for women: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	-0.386***	-18.05***	-0.773***	3.877**	0.0547**	-3.404***
	(0.0159)	(0.585)	(0.0283)	(1.345)	(0.0203)	(0.148)
First child aged 6-10=1	-0.303***	-17.55***	-0.735***	1.465	-0.00215	-3.518***
	(0.0177)	(0.702)	(0.0310)	(1.035)	(0.0206)	(0.189)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	0.0692***	3.239***	0.187***	-1.787	-0.0180	0.780***
	(0.0193)	(0.693)	(0.0334)	(1.559)	(0.0235)	(0.187)
First child aged 6-10=1 × First birth after 2010=1	0.0465+	2.620**	0.126**	-0.368	0.00631	0.722**
	(0.0247)	(0.931)	(0.0406)	(1.391)	(0.0269)	(0.267)
Pre-birth mean:						
Early cohorts	0.887	38.22	3.578	19.09	2.884	8.878
Late cohorts	0.909	37.20	3.537	24.69	3.143	9.375
Observations:						
N	116955	77959	77959	77959	72074	122094
N Individuals	12209	9501	9501	9501	9159	12879
N New Parents	1539	1386	1386	1386	1354	1539

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table reports the estimates for the child penalty in various labor market outcomes, both as baseline, for the cohorts of women who became mother for the first time before the Fair Work Act (early cohorts), and as change (difference between late cohorts and early cohorts). More formally, the displayed coefficients are estimated as in equation (3): the top panel reports estimates for the baseline child penalty (coefficients γ_{0-5} and γ_{6-10}), while the bottom panel reports estimates for the change in child penalty (coefficients $\gamma_{0-5,post}$ and $\gamma_{6-10,post}$). All outcome variables are defined in Appendix B.

Table 5: Time evolution of child penalty by job predictability

	(1) In labor force	(2) In labor force	(3) Weekly hours in paid employment (condi-tional)	(4) Weekly hours in paid employment (condi-tional)	(5) Log weekly hours in paid employment	(6) Log weekly hours in paid employment	(7) Regular schedule (M-F and regular daytime schedule)	(8) Regular schedule (M-F and regular daytime schedule)
Child penalty 0-5	-0.382***	-0.377***	-18.08***	-16.35***	-0.777***	-0.714***	-0.450***	-0.326***
	(0.0188)	(0.0245)	(0.676)	(0.863)	(0.0321)	(0.0406)	(0.0243)	(0.0311)
* above median SD		-0.00870		-3.961**		-0.145*		-0.282***
		(0.0384)		(1.364)		(0.0652)		(0.0462)
Child penalty 0-5 * post-2010 first birth	0.0962***	0.0955**	3.225***	0.770	0.197***	0.0827+	0.0777**	-0.00539
	(0.0225)	(0.0303)	(0.777)	(1.011)	(0.0369)	(0.0474)	(0.0295)	(0.0385)
* above median SD		0.000259		5.488***		0.255***		0.192***
		(0.0455)		(1.558)		(0.0745)		(0.0568)
Early (pre-2010) cohorts: Mean Y pre-birth	0.941		38.42		3.587		0.591	
- below median SD		0.932		37.09		3.537		0.459
- above median SD		0.953		40.24		3.655		0.772
Late (post-2010) cohorts: Mean Y pre-birth	0.961		37.43		3.551		0.604	
- below median SD		0.952		36.72		3.527		0.490
- above median SD		0.971		38.25		3.579		0.735
Early cohorts: New parents	371		367		367		367	
- below median SD		219		216		216		216
- above median SD		152		151		151		151
Late cohorts: New parents	675		673		673		673	
- below median SD		368		366		366		366
- above median SD		307		307		307		307
Tot observations	56647	56647	47500	47500	47500	47500	47588	47588

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the estimates for the child penalty in various labor market outcomes (columns 1 to 6) and in schedule predictability (columns 7 and 8), separately by exposure to the Fair Work Act. More formally, even-numbered columns report coefficients that are estimated from equation (5), i.e. γ_a (first row), $\gamma_{a,H}$ (second row), γ_b (third row) and $\gamma_{b,H}$. Odd-numbered columns report estimates from a more basic specification that does not distinguish between exposure groups, i.e. the analogous of the first and third rows from Table 4, but restricted to the subsample of respondents who answer the question about predictability in their schedule. All outcome variables are defined in Appendix B.

Table 6: The interaction between Paid Parental Leave and predictability

	(1) In labor force	(2) In labor force	(3) In labor force	(4) In labor force	(5) Log weekly hours in paid employment	(6) Log weekly hours in paid employment	(7) Log weekly hours in paid employment	(8) Log weekly hours in paid employment
First child aged 0-5	-0.351***	-0.352***	-0.321***	-0.321***	-0.785***	-0.733***	-0.763***	-0.701***
	(0.0230)	(0.0298)	(0.0278)	(0.0364)	(0.0399)	(0.0497)	(0.0472)	(0.0562)
× above median SD predict.		0.00399		-0.0000295		-0.120		-0.129
		(0.0468)		(0.0473)		(0.0816)		(0.0815)
× not PPL eligible			-0.0633	-0.0622			-0.0514	-0.0644
			(0.0463)	(0.0475)			(0.0825)	(0.0826)
First child aged 0-5 × First birth after 2010	0.0920***	0.100**	0.0536	0.0616	0.209***	0.109+	0.226***	0.120+
	(0.0271)	(0.0365)	(0.0331)	(0.0438)	(0.0455)	(0.0568)	(0.0538)	(0.0637)
× above median SD predict.		-0.0186		-0.0155		0.219*		0.218*
		(0.0546)		(0.0549)		(0.0926)		(0.0925)
× not PPL eligible			0.0769	0.0748			-0.0595	-0.0414
			(0.0548)	(0.0557)			(0.0957)	(0.0953)
Observations	35157	35157	35157	35157	30279	30279	30279	30279
N Individuals	2453	2453	2453	2453	2451	2451	2451	2451
N New Parents	668	668	668	668	667	667	667	667

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the estimates for the child penalty in labor supply at the extensive margin (columns 1-4) and at the intensive margin (columns 5-8), separately by exposure to the Fair Work Act and to the the Paid Parental Leave introduction. Columns 1 and 5 report simple estimates that do not control for exposure to any of the two measures; columns 2 and 6 account for exposure to the Fair Work Act by estimating the child penalty separately for mothers who were highly or lowly exposed to the law; columns 3 and 7 account for exposure to the introduction of Paid Parental Leave by estimating the child penalty for parents who were and were not already eligible; columns 4 and 8 account for both.

Table 7: Child penalties in time use for Women

	(1) Playing with children (hrs/week)	(2) Housework (hours/week)	(3) Paid employment (hrs/week)	(4) Other activities (hrs/week)	(5) Physical activity (hrs/week)	(6) Social activity (hrs/week)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	48.08***	9.183***	-24.38***	-2.814***	-0.343***	0.650***
	(0.948)	(0.443)	(0.658)	(0.200)	(0.0637)	(0.107)
First child aged 3-5=1	42.67***	11.38***	-23.56***	-2.171***	-0.482***	0.549***
	(1.019)	(0.514)	(0.754)	(0.236)	(0.0729)	(0.113)
First child aged 6-8=1	34.85***	11.35***	-22.24***	-2.058***	-0.278***	0.423***
	(0.983)	(0.575)	(0.827)	(0.278)	(0.0807)	(0.125)
First child aged 9-10=1	32.49***	10.32***	-21.01***	-1.811***	-0.255**	0.361**
	(0.988)	(0.565)	(0.905)	(0.331)	(0.0902)	(0.130)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	0.228	-1.593**	2.815***	0.150	-0.113	-0.210
	(1.221)	(0.532)	(0.815)	(0.259)	(0.0804)	(0.132)
First child aged 3-5=1 × First birth after 2010=1	0.184	-2.069**	3.557***	0.0109	-0.0476	-0.300*
	(1.286)	(0.634)	(0.934)	(0.327)	(0.0950)	(0.143)
First child aged 6-8=1 × First birth after 2010=1	2.511+	-2.495***	3.409**	0.354	-0.216+	-0.259
	(1.411)	(0.738)	(1.083)	(0.428)	(0.118)	(0.170)
First child aged 9-10=1 × First birth after 2010=1	3.918+	-0.286	3.214	-0.197	0.0619	-0.188
	(2.180)	(1.346)	(1.965)	(0.948)	(0.227)	(0.312)
Pre-birth mean:						
Early cohorts	0.448	9.319	31.77	3.540	2.568	3.659
Late cohorts	0.395	7.707	32.45	4.509	2.657	3.374
Observations:						
N	92991	98187	96944	122094	105587	104702
N Individuals	10973	11125	11087	12879	11624	11595
N New Parents	1504	1513	1510	1539	1518	1517

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the outcome variables are weekly hours spent in various non-labor market activities, and in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10. Similarly to Table 4 we are showing the child penalty both at baseline and in change. All outcome variables are defined in Appendix B.

A Appendix

A.1 Appendix figures

Note: This figure shows the frequency of Google Searches for the query "Fair Work Act", compared with the corresponding frequencies for other labor-market related queries – "Trade Union", "Disability Discrimination Act" and "Unfair dismissal", respectively. The series are scaled to be 100 on the day with the maximum number of queries, for the series in which this peak is higher. The time horizon covers the years from 2004 to 2020 (the Fair Work Act was approved in July 2009).

Figure A.2: “I am entitled to flexible start and finish times” for mothers relative to non-mothers, by year and age of child

Note: This figure represents the evolution over time of flexibility for women over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is “I am entitled to flexible start and finish times". The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimes for different ranges of child’s age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure A.3: Hours worked from home, for mothers relative to non-mothers, by year and age of child

Note: This figure represents the evolution over time of hours worked from home for women over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is the number of hours worked from home, conditional on being employed. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimes for different ranges of child's age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

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Figure A.4: “Regular schedule” for fathers relative to non-fathers, by year and age of child

Note: This figure represents the evolution over time of predictability for men over our sample years. More formally, we are plotting for each year the estimates of $\beta_{FC_{a,j}}$ and $\beta_{YC_{a,j}}$ from equation (1), where the outcome variable is “Regular Schedule", a dummy for working from Monday to Friday on a regular daytime schedule. The top panels, in blue, show the coefficients on the dummies based on the age of the first (oldest) child, $\beta_{FC_{a,j}}$, while the bottom panels, in green, show the coefficients on the dummies based the age of the youngest child, $\beta_{YC_{a,j}}$. From left to right, the panels display these estimes for different ranges of child’s age (0-2, 3-5, 6-8). The vertical dashed lines indicate the first cohort that includes at least some children born after the passage of the law (2010 for 0-2 year old, 2013 for 3-5 year old, and 2016 for 6-8 year old).

Figure A.5: Child penalty by gender: other labor market outcomes

(a) Log weekly hours in paid employment

(b) Log of (1 + annual labor income)

(c) Part time if employed

(d) Overtime (more than 50 hours/week)

(e) Permanent employment if employed (vs casual and fixed-term)

(f) Promoted in the past year

Note: This figure shows the child penalty in various labor market outcomes, as described in Section 4.1.1. Namely, the figure plots the coefficients γ_k's from estimating equation (2), which includes individual, time and age fixed effects. In all panels, the blue squares are estimates from a regression that includes only men, while the pink circles are estimates from a regression that includes only women. The shades around them are 95% confidence intervals. We only include parents who have their first child between 2003 and 2008, and we include non-parents as controls - see Section 4.1.1 for details. All regressions are estimated using the Sun and Abraham (2020) estimator, and clustering standard errors at the individual level. The outcome variables considered are defined in Appendix B.

Figure A.6: Change in the child penalty for women: other labor market outcomes

(a) Log weekly hours in paid employment

(b) Log of (1 + annual labor income)

(c) Part time if employed

(d) Overtime (more than 50 hours/week)

(e) Permanent employment if employed (vs casual and fixed-term)

(f) Promoted in the past year

Note: This figure shows how the child penalty in various labor market outcomes has changed, for women, over the sample period. All panels plot the child penalty in labor supply by year relative to childbirth (the coefficients γ_k's in equation (2)) for women, estimated separately for early cohorts (first childbirth between 2005 and 2008, green circles) and late cohorts (first childbirth between 2010 and 2013, red squares). Women without children are included in both regressions as controls, and women having children in other sample years are excluded from both regression. See note to Figure 3 for further details. The outcome variables considered are defined in Appendix B.

Figure A.7: Time evolution of the child penalty for women: other labor market outcomes

(a) Log weekly hours in paid employment

(b) Log of (1 + annual labor income)

(c) Part time if employed

(d) Permanent employment if employed (vs casual and fixed-term)

Note: This figure shows how the child penalty in various labor market outcomes has changed, for women, over the sample period. All panels display the 0-5 years child penalty (average of the child penalty from zero to five years after the birth of the first child) for women by year of birth of the first child. That is, they plot the estimates of $\gamma_{0-5,c}$ from equation (4). The blue circles are the estimates yearly cohort-by-yearly cohort, and the shades around them are 95% confidence intervals. The solid lines are the linear trends estimated for childbirth cohorts 2003-2008 and 2010-2017, and the dashed lines are the corresponding 95% confidence intervals, as described in Section 4.2.2. We cluster standard errors at the individual level. The outcome variables considered are defined in Appendix B.

Figure A.8: Time evolution of the child penalty for women (persistence)

(a) Labor force participation

(b) Weekly hours in paid employment (conditional on working)

(c) Hourly wage

(d) Log of (1 + weekly wages)

Note: This figure shows how the child penalty in labor supply, wages, and earnings for women has changed over the sample period. In panel (a) the outcome is a dummy for whether the individual is in the labor force; in panel (b) the outcome is weekly hours in paid employment conditional on working a strictly positive number of hours; in panel (c) the outcome is hourly wage, constructed by dividing weekly wages (winsorized at the 0.5% level, by year and gender) by the number of weekly hours in paid employment; in panel (d) the outcome is the natural logarithm of 1 + weekly wages, again winsorised. All panels display the 0-5 and the 6-10 years child penalty (average of the child penalty from zero to five years, and from six to ten years, after the birth of the first child) for women by year of birth of the first child. That is, they plot the estimates of $\gamma_{0-5,c}$ and $\gamma_{6-10,c}$ from equation (4). The circles are the estimates yearly cohort-by-yearly cohort, and the shades around them are 95% confidence intervals. We cluster standard errors at the individual level.

Figure A.9: Permutation test

(a) Labor force participation

(b) Weekly hours in paid employment (conditional)

(c) Log of Hourly wage

(d) Log of (1 + weekly wages)

(e) Log weekly hours in paid employment

(f) Log of (1 + annual labor income)

Figure A.10: Testing for break in the trend

(a) Labor force participation

(b) Weekly hours in paid employment (conditional on working)

(c) Log of Hourly wage

(d) Log of (1 + weekly wages)

(e) Log of weekly hours in paid employment

(f) Log of (1 + annual income)

Figure A.11: Distribution of standard deviation of predictability

Figure A.12: The change in child penalty is increasing in the standard deviation of predictability

Note: This figure displays the relationship between the standard deviation of predictability (our measure of exposure to the Fair Work Act) and the change in child penalty in the intenstive margin of labor supply (weekly hours in paid employment). The latter is defined as the coefficient $\gamma_{0-5,post}$ in equation (3).

A.2 Appendix tables

Table A.1: Balance Table: Occupation and Industry of Early vs Late Cohorts of Mothers

	Early cohort (2003-2008)			Late cohort (2010-2017)				
	N	Mean	SD	N	Mean	SD	Diff.	SE
Occupations								
Managers	375	0.11	(0.32)	699	0.11	(0.32)	-0.00	(0.02)
Professionals	375	0.33	(0.47)	699	0.37	(0.48)	0.05	(0.03)
Technicians and Trade Workers	375	0.06	(0.25)	699	0.04	(0.20)	-0.02	(0.01)
Community and Personal Service Workers	375	0.12	(0.32)	699	0.14	(0.35)	0.02	(0.02)
Clerical and Admin Workers	375	0.19	(0.39)	699	0.20	(0.40)	0.01	(0.03)
Sales Workers	375	0.13	(0.33)	699	0.09	(0.28)	-0.04*	(0.02)
Machinery Operators and Drivers	375	0.01	(0.09)	699	0.00	(0.07)	-0.00	(0.00)
Labourers	375	0.05	(0.22)	699	0.04	(0.19)	-0.02	(0.01)
Industries								
Agriculture, Forestry and Fishing	373	0.01	(0.10)	696	0.01	(0.10)	-0.00	(0.01)
Mining	373	0.01	(0.10)	696	0.01	(0.11)	0.00	(0.01)
Manifacturing	373	0.06	(0.24)	696	0.04	(0.19)	-0.02	(0.01)
Electricity, Gas, Water, Waste	373	0.01	(0.07)	696	0.00	(0.07)	-0.00	(0.00)
Construction	373	0.02	(0.13)	696	0.02	(0.15)	0.01	(0.01)
Wholesale Trade	373	0.02	(0.14)	696	0.02	(0.15)	0.01	(0.01)
Retail Trade	373	0.13	(0.33)	696	0.09	(0.28)	-0.04*	(0.02)
Accommodation and Food Services	373	0.09	(0.29)	696	0.08	(0.27)	-0.02	(0.02)
Trasport, Postal, Wharehousing	373	0.02	(0.13)	696	0.02	(0.12)	-0.00	(0.01)
Information Media and Telecommunications	373	0.03	(0.17)	696	0.02	(0.15)	-0.01	(0.01)
Financial and Insurance Services	373	0.05	(0.21)	696	0.05	(0.23)	0.01	(0.01)
Rental, Hiring, Real Estate	373	0.02	(0.13)	696	0.02	(0.13)	0.00	(0.01)
Professional, Scientific and Technical Services	373	0.09	(0.29)	696	0.10	(0.30)	0.01	(0.02)
Admin and Support Services	373	0.02	(0.15)	696	0.04	(0.19)	0.02	(0.01)
Public Admin and Safety	373	0.07	(0.26)	696	0.07	(0.25)	-0.00	(0.02)
Education and Training	373	0.13	(0.34)	696	0.14	(0.35)	0.01	(0.02)
Healthcare and Social Assistance	373	0.18	(0.38)	696	0.21	(0.41)	0.04	(0.03)
Arts and Recreation Services	373	0.02	(0.15)	696	0.02	(0.13)	-0.00	(0.01)
Other Services	373	0.03	(0.18)	696	0.04	(0.19)	0.00	(0.01)

Note: See note to Table 2. Each raw represents a dummy variable taking the value of one if the individual works in the given occupation / industry.

Table A.2: Changes in working arrangements for fathers

	Predictability				Flexibility		Work from home			
	(1) Regular schedule (M-F and regular daytime schedule)	(2) M-F	(3) Regular daytime schedule	(4) On call and irregular shifts	(5) My working times can be flexible (yes-no)	(6) Entitled to flexible start/finish times	(7) Hours work from home	(8) Any hours worked from home	(9) Hours work from home conditional on any	(10) Entitled to home-based work
First child 0-2	0.0154	0.0177	0.00520	0.00375	0.0273	0.0249	0.0123	-0.0136	0.606	0.0216
× First birth after 2010	(0.0184)	(0.0182)	(0.0156)	(0.0110)	(0.0217)	(0.0207)	(0.257)	(0.0160)	(0.848)	(0.0181)
First child 3-5	0.0151	0.0176	0.00928	-0.00981	0.0189	-0.0444*	-0.191	-0.000137	-1.054	-0.0185
× First birth after 2013	(0.0215)	(0.0213)	(0.0175)	(0.0124)	(0.0229)	(0.0219)	(0.337)	(0.0189)	(0.947)	(0.0214)
First child 6-8	-0.00850	-0.00643	0.00631	-0.00306	-0.00566	0.000884	0.0148	0.0165	-0.676	0.0152
× First birth after 2016	(0.0259)	(0.0252)	(0.0210)	(0.0136)	(0.0254)	(0.0251)	(0.442)	(0.0216)	(1.059)	(0.0225)
Pre-period mean:										
First child aged 0-2	0.555	0.604	0.790	0.0789	0.510	0.587	2.446	0.251	9.826	0.252
First child aged 3-5	0.589	0.625	0.802	0.0867	0.519	0.645	2.880	0.274	10.67	0.309
First child aged 6-8	0.618	0.652	0.798	0.0863	0.581	0.665	3.475	0.288	12.17	0.314
Observations:										
N	89605	89617	89621	89621	62311	66164	98520	98520	20737	61043
N Individuals	10063	10063	10065	10065	8200	8397	11182	11182	3494	8130

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table shows estimates analogous to those in Table 3, for the subsample of fathers. See note to Table 3.

Table A.3: Child penalties for men: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	-0.0132	-1.782***	-0.0573***	0.961+	0.0424**	-0.0439
	(0.00833)	(0.390)	(0.0128)	(0.547)	(0.0140)	(0.115)
First child aged 3-5=1	-0.0217*	-1.505**	-0.0525***	1.376+	0.0370*	-0.219
	(0.00984)	(0.485)	(0.0148)	(0.740)	(0.0167)	(0.146)
First child aged 6-8=1	-0.0133	-1.783***	-0.0629***	2.199*	0.0357+	-0.209
	(0.0111)	(0.533)	(0.0171)	(1.020)	(0.0198)	(0.167)
First child aged 9-10=1	-0.0248+	-2.395***	-0.0830***	2.861*	0.0296	-0.112
	(0.0129)	(0.626)	(0.0195)	(1.165)	(0.0239)	(0.186)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	-0.00863	0.436	0.0109	0.549	-0.0166	-0.108
	(0.0103)	(0.504)	(0.0161)	(0.745)	(0.0179)	(0.149)
First child aged 3-5=1 × First birth after 2010=1	0.00519	-0.332	-0.0210	0.228	-0.0140	0.0343
	(0.0121)	(0.610)	(0.0190)	(1.013)	(0.0218)	(0.186)
First child aged 6-8=1 × First birth after 2010=1	-0.0187	-0.770	-0.0403	-0.600	-0.0229	-0.0867
	(0.0156)	(0.790)	(0.0247)	(1.418)	(0.0264)	(0.242)
First child aged 9-10=1 × First birth after 2010=1	0.0181	-0.857	-0.0301	0.0930	-0.0465	-0.0864
	(0.0242)	(1.201)	(0.0329)	(2.838)	(0.0572)	(0.373)
Pre-birth mean:						
Early cohorts	0.952	44.35	3.737	20.52	2.963	9.220
Late cohorts	0.952	43.55	3.720	27.85	3.277	9.627
Observations:						
N	107225	84018	84018	84018	74203	116055
N Individuals	11732	9802	9802	9802	9279	12748
N New Parents	1338	1302	1302	1302	1281	1338

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the subsample consider is fathers, and in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10. See note to Table 4 for additional details.

Table A.4: Child penalties for women: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	-0.398***	-18.01***	-0.777***	5.524**	0.0909***	-2.843***
	(0.0167)	(0.609)	(0.0303)	(2.003)	(0.0236)	(0.140)
First child aged 3-5=1	-0.369***	-18.19***	-0.772***	2.099	0.0159	-4.098***
	(0.0188)	(0.702)	(0.0336)	(1.384)	(0.0249)	(0.191)
First child aged 6-8=1	-0.309***	-18.07***	-0.758***	1.841	0.0124	-3.777***
	(0.0187)	(0.734)	(0.0331)	(1.150)	(0.0225)	(0.202)
First child aged 9-10=1	-0.281***	-16.99***	-0.709***	0.339	-0.0378	-3.364***
	(0.0202)	(0.778)	(0.0340)	(1.132)	(0.0234)	(0.210)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	0.0608**	4.015***	0.225***	-3.600	-0.0547*	0.419*
	(0.0206)	(0.729)	(0.0361)	(2.220)	(0.0272)	(0.179)
First child aged 3-5=1 × First birth after 2010=1	0.0936***	2.138*	0.136***	0.0482	0.0167	1.064***
	(0.0232)	(0.840)	(0.0397)	(1.676)	(0.0291)	(0.244)
First child aged 6-8=1 × First birth after 2010=1	0.0639*	2.727**	0.131**	-0.745	-0.0103	0.846**
	(0.0256)	(0.965)	(0.0429)	(1.522)	(0.0293)	(0.279)
First child aged 9-10=1 × First birth after 2010=1	0.0331	3.540*	0.168**	-0.741	0.0104	0.770
	(0.0452)	(1.500)	(0.0567)	(2.091)	(0.0458)	(0.472)
Pre-birth mean:						
Early cohorts	0.887	38.22	3.578	19.09	2.884	8.878
Late cohorts	0.909	37.20	3.537	24.69	3.143	9.375
Observations:						
N	116955	77959	77959	77959	72074	122094
N Individuals	12209	9501	9501	9501	9159	12879
N New Parents	1539	1386	1386	1386	1354	1539

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the subsample consider is mothers, and in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10. See note to Table 4 for additional details.

Table A.5: Child penalties for women: other labor market outcomes

	(1) Part-time if employed	(2) Overtime (more than 38 hours/week)	(3) Overtime (more than 50 hours/week)	(4) Permanent emplyment if employed (vs casual and fixed-term)	(5) Normally supervise work of other employees	(6) Promoted in the past year (conditional on current employment)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	0.600***	-0.392***	-0.103***	-0.194***	-0.124***	-0.104***
	(0.0219)	(0.0157)	(0.00946)	(0.0228)	(0.0225)	(0.0153)
First child aged 3-5=1	0.632***	-0.407***	-0.106***	-0.273***	-0.154***	-0.0802***
	(0.0243)	(0.0182)	(0.0109)	(0.0255)	(0.0240)	(0.0161)
First child aged 6-8=1	0.634***	-0.402***	-0.107***	-0.274***	-0.152***	-0.0819***
	(0.0271)	(0.0193)	(0.0109)	(0.0278)	(0.0252)	(0.0161)
First child aged 9-10=1	0.619***	-0.379***	-0.104***	-0.245***	-0.142***	-0.0624**
	(0.0297)	(0.0213)	(0.0119)	(0.0290)	(0.0287)	(0.0195)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	-0.0611*	0.0548**	0.0233*	0.125***	0.0494+	0.0178
	(0.0269)	(0.0197)	(0.0111)	(0.0272)	(0.0277)	(0.0185)
First child aged 3-5=1 × First birth after 2010=1	-0.00293	0.0396+	0.0232+	0.131***	0.0448	-0.00992
	(0.0303)	(0.0226)	(0.0131)	(0.0319)	(0.0310)	(0.0198)
First child aged 6-8=1 × First birth after 2010=1	-0.0437	0.0575*	0.0298*	0.0978**	0.0539	0.00906
	(0.0373)	(0.0269)	(0.0146)	(0.0371)	(0.0360)	(0.0237)
First child aged 9-10=1 × First birth after 2010=1	-0.0667	0.0743	0.0734*	0.0662	0.0392	-0.0340
	(0.0601)	(0.0474)	(0.0345)	(0.0755)	(0.0650)	(0.0470)
Pre-birth mean:						
Early cohorts	0.221	0.440	0.110	0.716	0.498	0.159
Late cohorts	0.226	0.399	0.0808	0.701	0.446	0.157
Observations:						
N	78031	122094	122094	69088	78142	67308
N Individuals	9501	12879	12879	8846	9504	8719
N New Parents	1386	1539	1539	1342	1387	1340

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table A.4 but considers different labor market outcomes. All variables are defined in Appendix B.

Table A.6: Child penalties for women: working arrangements (continues in next page)

	Predictability				Flexibility	
	(1)	(2)	(3)	(4)	(5)	(6)
	Regular schedule (M-F and regular daytime schedule)	M-F	Regular daytime schedule	On call and irregular shifts	My working times can be flexible (yes-no)	Entitled to flexible start/finish times
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	-0.427***	-0.421***	-0.116***	0.0752***	0.152***	0.0316
	(0.0222)	(0.0227)	(0.0188)	(0.0152)	(0.0295)	(0.0219)
First child aged 3-5=1	-0.462***	-0.461***	-0.142***	0.101***	0.163***	0.0378
	(0.0250)	(0.0254)	(0.0212)	(0.0164)	(0.0309)	(0.0248)
First child aged 6-8=1	-0.475***	-0.458***	-0.124***	0.0815***	0.161***	0.0337
	(0.0270)	(0.0279)	(0.0211)	(0.0152)	(0.0333)	(0.0257)
First child aged 9-10=1	-0.427***	-0.419***	-0.134***	0.0761***	0.154***	0.0233
	(0.0296)	(0.0307)	(0.0222)	(0.0157)	(0.0355)	(0.0289)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	0.0933***	0.0886**	0.0700**	-0.0380*	-0.0463	0.00973
	(0.0276)	(0.0278)	(0.0219)	(0.0174)	(0.0338)	(0.0264)
First child aged 3-5=1 × First birth after 2010=1	0.0679*	0.0730*	0.0876***	-0.0550**	-0.0162	0.0218
	(0.0322)	(0.0323)	(0.0254)	(0.0197)	(0.0357)	(0.0298)
First child aged 6-8=1 × First birth after 2010=1	0.130**	0.115**	0.0797**	-0.0369+	-0.00705	0.0414
	(0.0397)	(0.0403)	(0.0284)	(0.0210)	(0.0421)	(0.0332)
First child aged 9-10=1 × First birth after 2010=1	0.0949	0.0906	0.120**	-0.0637*	0.000206	0.0770
	(0.0647)	(0.0660)	(0.0413)	(0.0285)	(0.0637)	(0.0539)
Pre-birth mean:						
Early cohorts	0.597	0.625	0.803	0.0659	0.411	0.563
Late cohorts	0.599	0.627	0.802	0.0567	0.433	0.590
Observations:						
N	78115	78121	78140	78140	57256	60698
N Individuals	9503	9504	9504	9504	7979	8220
N New Parents	1387	1387	1387	1387	1290	1289

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: Child penalties for women: working arrangements (continues from previous page)

	Work from home			
	(7)	(8)	(9)	(10)
	Hours work from home	Any hours worked from home	Hours work from home conditional on any	Entitled to home-based work
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)				
First child aged 0-2=1	-0.0837	0.0476*	-3.204***	0.0360+
	(0.266)	(0.0198)	(0.944)	(0.0200)
First child aged 3-5=1	0.164	0.0549**	-2.258*	0.0420+
	(0.364)	(0.0207)	(1.020)	(0.0232)
First child aged 6-8=1	0.285	0.0511*	-2.077+	-0.00630
	(0.377)	(0.0220)	(1.196)	(0.0243)
First child aged 9-10=1	0.566	0.0462+	-1.400	-0.0185
	(0.445)	(0.0239)	(1.179)	(0.0266)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)				
First child aged 0-2=1 × First birth after 2010=1	0.492	-0.0285	2.413*	-0.0283
	(0.329)	(0.0244)	(1.138)	(0.0247)
First child aged 3-5=1 × First birth after 2010=1	0.771+	0.0103	1.797	0.000375
	(0.449)	(0.0270)	(1.301)	(0.0285)
First child aged 6-8=1 × First birth after 2010=1	1.106+	0.0564+	1.651	0.0178
	(0.582)	(0.0319)	(1.710)	(0.0335)
First child aged 9-10=1 × First birth after 2010=1	1.388	0.0927	2.098	0.0829
	(1.163)	(0.0638)	(2.013)	(0.0536)
Pre-birth mean:				
Early cohorts	1.462	0.176	8.503	0.230
Late cohorts	1.506	0.183	8.382	0.270
Observations:				
N	83118	83118	16667	56410
N Individuals	10251	10251	2991	7973
N New Parents	1395	1395	593	1273

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Tables A.4 and A.5, but considers as outcomes various working arrangements: measures of predictability (columns 1 to 4), of flexibility (columns 5 and 6), and work from home (columns 7 to 10). All variables are defined in Appendix B.

Table A.7: Child penalties for women: index measures of (in)flexibility

	(1) Time pressure	(2) Importance of relationships	(3) Unstructured work	(4) Freedom to make decisions	(5) Inflexible job	(6) Freedom in decision making	(7) Time pressure, complex and stressful job
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)							
First child aged 0-2=1	-0.122***	-0.249***	-0.0784*	-0.105**	-0.146***	-0.242**	-0.658***
	(0.0357)	(0.0451)	(0.0397)	(0.0393)	(0.0286)	(0.0904)	(0.0699)
First child aged 3-5=1	-0.120**	-0.309***	-0.0730+	-0.121**	-0.155***	-0.310**	-0.654***
	(0.0422)	(0.0482)	(0.0434)	(0.0392)	(0.0290)	(0.0961)	(0.0788)
First child aged 6-8=1	-0.0859+	-0.305***	-0.135**	-0.121**	-0.182***	-0.377***	-0.564***
	(0.0444)	(0.0506)	(0.0473)	(0.0418)	(0.0292)	(0.105)	(0.0863)
First child aged 9-10=1	-0.126**	-0.284***	-0.118*	-0.161***	-0.171***	-0.366**	-0.560***
	(0.0462)	(0.0576)	(0.0502)	(0.0476)	(0.0331)	(0.116)	(0.0994)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)							
First child aged 0-2=1 × First birth after 2010=1	0.0282	0.0991+	0.0746	0.0292	0.0457	0.134	0.114
	(0.0409)	(0.0556)	(0.0476)	(0.0457)	(0.0337)	(0.108)	(0.0886)
First child aged 3-5=1 × First birth after 2010=1	-0.0233	0.0899	0.0769	0.0568	0.0289	0.204+	-0.00463
	(0.0482)	(0.0621)	(0.0534)	(0.0476)	(0.0358)	(0.121)	(0.101)
First child aged 6-8=1 × First birth after 2010=1	-0.0300	0.108	0.129*	0.0608	0.0557	0.358*	0.0193
	(0.0542)	(0.0723)	(0.0619)	(0.0547)	(0.0401)	(0.144)	(0.120)
First child aged 9-10=1 × First birth after 2010=1	0.00518	0.0787	0.203+	0.146	0.102	0.409+	-0.188
	(0.0829)	(0.130)	(0.108)	(0.0931)	(0.0740)	(0.237)	(0.219)
Pre-birth mean:							
Early cohorts	0.179	0.0737	-0.0330	-0.0489	0.0271	-0.404	-0.0194
Late cohorts	0.184	-0.0310	-0.131	-0.00959	0.00146	-0.596	0.0523
Observations:							
N	69604	78142	69608	69598	79406	67843	67843
N Individuals	8914	9504	8916	8915	9603	8777	8777
N New Parents	1337	1387	1337	1337	1397	1323	1323

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table reports the coefficient estimates from a version of equation (3), in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10.

Table A.8: Child penalties for women: change in flexibility from changing job

	(1) Regular schedule (job average)	(2) Flexible working times (job average)	(3) Entitlement to flex entry/exit times (job average)	(4) Inflexible job (job average)	(5) Freedom in decision making (job average)	(6) Time pressure (job average)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	-0.0346***	-0.00874	-0.00814	-0.0455***	-0.165***	-0.122***
	(0.00864)	(0.0140)	(0.00663)	(0.00894)	(0.0367)	(0.0238)
First child aged 3-5=1	-0.0534***	-0.00354	-0.0129	-0.0541***	-0.225***	-0.176***
	(0.00943)	(0.0168)	(0.00820)	(0.0101)	(0.0422)	(0.0263)
First child aged 6-8=1	-0.0581***	-0.0332+	-0.0258**	-0.0726***	-0.301***	-0.195***
	(0.0105)	(0.0189)	(0.00935)	(0.0121)	(0.0479)	(0.0302)
First child aged 9-10=1	-0.0591***	-0.0388+	-0.0214*	-0.0748***	-0.318***	-0.206***
	(0.0114)	(0.0205)	(0.0104)	(0.0126)	(0.0523)	(0.0333)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	0.0108	0.00760	0.000783	0.0237*	0.0570	0.0547+
	(0.0102)	(0.0172)	(0.00802)	(0.0111)	(0.0448)	(0.0285)
First child aged 3-5=1 × First birth after 2010=1	0.0185	0.0140	0.00818	0.0226+	0.0925+	0.0596+
	(0.0117)	(0.0215)	(0.0101)	(0.0129)	(0.0531)	(0.0340)
First child aged 6-8=1 × First birth after 2010=1	0.0114	0.0409	0.0131	0.0446**	0.187**	0.0562
	(0.0147)	(0.0256)	(0.0126)	(0.0156)	(0.0647)	(0.0425)
First child aged 9-10=1 × First birth after 2010=1	0.0124	0.000752	0.000300	0.0476+	0.226*	0.0779
	(0.0240)	(0.0509)	(0.0247)	(0.0274)	(0.0969)	(0.0793)
Pre-birth mean:						
Early cohorts	0.448	-0.00871	0.581	0.0138	-0.245	-0.0814
Late cohorts	0.453	-0.000979	0.585	0.0116	-0.219	-0.0755
Observations:						
N	75995	75459	75723	75995	75894	75894
N Individuals	9435	9403	9418	9435	9427	9427
N New Parents	1377	1376	1377	1377	1377	1377

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

This table reports the coefficient estimates from a version of equation (3), in which the child's age is split in finer groups (four as opposed to two): first child aged 0-2, 3-5, 6-8, 9-10.

Table A.9: Child penalties for men: main labor market outcomes

	(1) In labor force	(2) Weekly hours in paid employment (conditional)	(3) Log weekly hours in paid employment	(4) Hourly wage	(5) Log hourly wage	(6) Log(1+annual labor income)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	-0.0172*	-1.654***	-0.0549***	1.145+	0.0400**	-0.123
	(0.00844)	(0.393)	(0.0128)	(0.587)	(0.0139)	(0.120)
First child aged 6-10=1	-0.0173	-2.016***	-0.0698***	2.412*	0.0339+	-0.160
	(0.0110)	(0.527)	(0.0169)	(0.970)	(0.0195)	(0.161)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	-0.00252	0.141	-0.000725	0.379	-0.0151	-0.0389
	(0.0102)	(0.497)	(0.0160)	(0.784)	(0.0177)	(0.151)
First child aged 6-10=1 × First birth after 2010=1	-0.0130	-0.505	-0.0295	-0.758	-0.0227	-0.122
	(0.0152)	(0.762)	(0.0238)	(1.405)	(0.0267)	(0.232)
Pre-birth mean:						
Early cohorts	0.952	44.35	3.737	20.52	2.963	9.220
Late cohorts	0.952	43.55	3.720	27.85	3.277	9.627
Observations:						
N	107225	84018	84018	84018	74203	116055
N Individuals	11732	9802	9802	9802	9279	12748
N New Parents	1338	1302	1302	1302	1281	1338

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 4, but applies to the subsample of men

Table A.10: Child penalties for women: other labor market outcomes

	(1) Part-time if employed	(2) Overtime (more than 38 hours/week)	(3) Overtime (more than 50 hours/week)	(4) Permanent emplyment if employed (vs casual and fixed-term)	(5) Normally supervise work of other employees	(6) Promoted in the past year (conditional on current employment)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	0.614***	-0.398***	-0.105***	-0.229***	-0.138***	-0.0923***
	(0.0205)	(0.0160)	(0.00968)	(0.0212)	(0.0209)	(0.0141)
First child aged 6-10=1	0.622***	-0.391***	-0.106***	-0.254***	-0.145***	-0.0758***
	(0.0262)	(0.0191)	(0.0106)	(0.0259)	(0.0246)	(0.0160)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	-0.0398	0.0504*	0.0234*	0.133***	0.0499+	0.00391
	(0.0250)	(0.0197)	(0.0113)	(0.0256)	(0.0260)	(0.0168)
First child aged 6-10=1 × First birth after 2010=1	-0.0486	0.0534*	0.0321*	0.0895*	0.0516	0.000212
	(0.0361)	(0.0266)	(0.0146)	(0.0355)	(0.0351)	(0.0235)
Pre-birth mean:						
Early cohorts	0.221	0.440	0.110	0.716	0.498	0.159
Late cohorts	0.226	0.399	0.0808	0.701	0.446	0.157
Observations:						
N	78031	122094	122094	69088	78142	67308
N Individuals	9501	12879	12879	8846	9504	8719
N New Parents	1386	1539	1539	1342	1387	1340

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 4, but applies to different labor market outcomes. All variables are defined in Appendix B.

Table A.11: Child penalties for women: working arrangements (continues in next page)

	Predictability				Flexibility	
	(1)	(2)	(3)	(4)	(5)	(6)
	Regular schedule (M-F and regular daytime schedule)	M-F	Regular daytime schedule	On call and irregular shifts	My working times can be flexible (yes-no)	Entitled to flexible start/finish times
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	-0.442***	-0.439***	-0.128***	0.0872***	0.156***	0.0341+
	(0.0213)	(0.0218)	(0.0177)	(0.0138)	(0.0275)	(0.0205)
First child aged 6-10=1	-0.451***	-0.438***	-0.126***	0.0774***	0.154***	0.0281
	(0.0258)	(0.0268)	(0.0200)	(0.0140)	(0.0319)	(0.0248)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	0.0848**	0.0847**	0.0796***	-0.0470**	-0.0347	0.0145
	(0.0265)	(0.0267)	(0.0204)	(0.0159)	(0.0312)	(0.0245)
First child aged 6-10=1 × First birth after 2010=1	0.115**	0.104**	0.0860**	-0.0370+	-0.00775	0.0460
	(0.0381)	(0.0387)	(0.0267)	(0.0194)	(0.0404)	(0.0321)
Pre-birth mean:						
Early cohorts	0.597	0.625	0.803	0.0659	0.411	0.563
Late cohorts	0.599	0.627	0.802	0.0567	0.433	0.590
Observations:						
N	78115	78121	78140	78140	57256	60698
N Individuals	9503	9504	9504	9504	7979	8220
N New Parents	1387	1387	1387	1387	1290	1289

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.11: Child penalties for women: working arrangements (continues from previous page)

	Work from home			
	(7)	(8)	(9)	(10)
	Hours work from home	Any hours worked from home	Hours work from home conditional on any	Entitled to home-based work
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)				
First child aged 0-5=1	0.0284	0.0503**	-2.773**	0.0380*
	(0.263)	(0.0178)	(0.843)	(0.0191)
First child aged 6-10=1	0.358	0.0468*	-1.987+	-0.0135
	(0.373)	(0.0207)	(1.101)	(0.0231)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)				
First child aged 0-5=1 × First birth after 2010=1	0.579+	-0.0131	2.053+	-0.0167
	(0.323)	(0.0224)	(1.049)	(0.0233)
First child aged 6-10=1 × First birth after 2010=1	0.982+	0.0563+	1.457	0.0234
	(0.587)	(0.0310)	(1.581)	(0.0320)
Pre-birth mean:				
Early cohorts	1.462	0.176	8.503	0.230
Late cohorts	1.506	0.183	8.382	0.270
Observations:				
N	83118	83118	16667	56410
N Individuals	10251	10251	2991	7973
N New Parents	1395	1395	593	1273

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table A.6, but the child's age is split in coarser groups (two as opposed to four): first child aged 0-5 and 6-10.

Table A.12: Child penalties for women: index measures of (in)flexibility

	(1) Time pressure	(2) Importance of relationships	(3) Unstruc-tured work	(4) Freedom to make decisions	(5) Inflexible job	(6) Freedom in decision making	(7) Time pressure, complex and stressful job
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)							
First child aged 0-5=1	-0.120***	-0.276***	-0.0757*	-0.113**	-0.150***	-0.276***	-0.654***
	(0.0354)	(0.0420)	(0.0370)	(0.0347)	(0.0253)	(0.0824)	(0.0671)
First child aged 6-10=1	-0.0994*	-0.290***	-0.129**	-0.137***	-0.176***	-0.369***	-0.557***
	(0.0422)	(0.0493)	(0.0449)	(0.0415)	(0.0284)	(0.101)	(0.0834)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)							
First child aged 0-5=1 × First birth after 2010=1	0.00752	0.100+	0.0747+	0.0432	0.0395	0.170+	0.0631
	(0.0399)	(0.0522)	(0.0442)	(0.0406)	(0.0301)	(0.100)	(0.0840)
First child aged 6-10=1 × First birth after 2010=1	-0.00770	0.104	0.128*	0.0797	0.0575	0.355*	0.0137
	(0.0516)	(0.0704)	(0.0592)	(0.0536)	(0.0392)	(0.138)	(0.115)
Pre-birth mean:							
Early cohorts	0.179	0.0737	-0.0330	-0.0489	0.0271	-0.404	-0.0194
Late cohorts	0.184	-0.0310	-0.131	-0.00959	0.00146	-0.596	0.0523
Observations:							
N	69604	78142	69608	69598	79406	67843	67843
N Individuals	8914	9504	8916	8915	9603	8777	8777
N New Parents	1337	1387	1337	1337	1397	1323	1323

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.13: Child penalties for women: change in flexibility from changing job

	(1) Regular schedule (job average)	(2) Flexible working times (job average)	(3) Entitlement to flex entry/exit times (job average)	(4) Inflexible job (job average)	(5) Freedom in decision making (job average)	(6) Time pressure (job average)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	-0.0434***	-0.00650	-0.0104	-0.0495***	-0.193***	-0.147***
	(0.00798)	(0.0135)	(0.00664)	(0.00840)	(0.0349)	(0.0222)
First child aged 6-10=1	-0.0568***	-0.0362*	-0.0239**	-0.0724***	-0.302***	-0.193***
	(0.00987)	(0.0180)	(0.00903)	(0.0114)	(0.0459)	(0.0294)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	0.0155	0.00991	0.00409	0.0240*	0.0769+	0.0614*
	(0.00947)	(0.0167)	(0.00790)	(0.0104)	(0.0428)	(0.0268)
First child aged 6-10=1 × First birth after 2010=1	0.0123	0.0388	0.0102	0.0463**	0.196**	0.0649
	(0.0141)	(0.0246)	(0.0121)	(0.0150)	(0.0624)	(0.0417)
Pre-birth mean:						
Early cohorts	0.448	-0.00871	0.581	0.0138	-0.245	-0.0814
Late cohorts	0.453	-0.000979	0.585	0.0116	-0.219	-0.0755
Observations:						
N	75995	75459	75723	75995	75894	75894
N Individuals	9435	9403	9418	9435	9427	9427
N New Parents	1377	1376	1377	1377	1377	1377

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.14: Permutation test

	P-value asymptotic	P-value random
In labor force	.290	.36
Weekly hrs in paid emp (cond)	.004	.18
Log weekly h in paid emp (cond)	.036	.18
Log(1+weekly wage)	.026	.18
Log(1+annual labor income)	.002	.18
Log (hourly wage)	.546	.73

Table A.15: Time evolution of child penalty by job predictability (short and long run, 1/2)

	(1) Weekly hours in paid employment (condi-tional)	(2) Weekly hours in paid employment (condi-tional)	(3) Regular schedule (M-F and regular daytime schedule)	(4) Regular schedule (M-F and regular daytime schedule)
Child penalty 0-5	-18.08***	-16.35***	-0.450***	-0.326***
	(0.676)	(0.863)	(0.0243)	(0.0311)
* above median SD		-3.961**		-0.282***
		(1.364)		(0.0462)
Child penalty 0-5 * post-2010 first birth	3.225***	0.770	0.0777**	-0.00539
	(0.777)	(1.011)	(0.0295)	(0.0385)
* above median SD		5.488***		0.192***
		(1.558)		(0.0568)
Child penalty 6-10	-17.69***	-16.43***	-0.464***	-0.324***
	(0.805)	(1.086)	(0.0300)	(0.0386)
* above median SD		-2.854+		-0.315***
		(1.609)		(0.0577)
Child penalty 6-10 * post-2010 first birth	3.317**	1.546	0.131**	0.0314
	(1.032)	(1.429)	(0.0438)	(0.0592)
* above median SD		3.960+		0.228**
		(2.059)		(0.0855)
Early (pre-2010) cohorts: Mean Y pre-birth	38.42		0.591	
- below median SD		37.09		0.459
- above median SD		40.24		0.772
Late (post-2010) cohorts: Mean Y pre-birth	37.43		0.604	
- below median SD		36.72		0.490
- above median SD		38.25		0.735
Early cohorts: New parents	367		367	
- below median SD		216		216
- above median SD		151		151
Late cohorts: New parents	673		673	
- below median SD		366		366
- above median SD		307		307
Early cohorts: Observations	3867		3872	
- below median SD		2209		2213
- above median SD		1658		1659
Late cohorts: Observations	5997		6009	
- below median SD		3242		3249
- above median SD		2755		2760
Tot observations	47500	47500	47588	47588

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.16: Time evolution of child penalty by job predictability (short and long run, 2/2)

	(1) Part-time if employed	(2) Part-time if employed	(3) Promoted in the past year (conditional on current employ-ment)	(4) Promoted in the past year (conditional on current employ-ment)
Child penalty 0-5	0.609***	0.527***	-0.0960***	-0.0716***
	(0.0240)	(0.0324)	(0.0169)	(0.0211)
* above median SD		0.186***		-0.0592+
		(0.0470)		(0.0342)
Child penalty 0-5 * post-2010 first birth	-0.0238	0.0667+	0.0144	-0.0318
	(0.0282)	(0.0388)	(0.0196)	(0.0252)
* above median SD		-0.201***		0.107**
		(0.0553)		(0.0394)
Child penalty 6-10	0.631***	0.554***	-0.0806***	-0.0854***
	(0.0304)	(0.0414)	(0.0180)	(0.0226)
* above median SD		0.174**		0.00759
		(0.0602)		(0.0367)
Child penalty 6-10 * post-2010 first birth	-0.0668	0.00516	0.0294	-0.00447
	(0.0409)	(0.0571)	(0.0265)	(0.0331)
* above median SD		-0.162*		0.0742
		(0.0813)		(0.0535)
Early (pre-2010) cohorts: Mean Y pre-birth	0.219		0.162	
- below median SD		0.282		0.145
- above median SD		0.134		0.185
Late (post-2010) cohorts: Mean Y pre-birth	0.218		0.158	
- below median SD		0.251		0.173
- above median SD		0.180		0.140
Early cohorts: New parents	367		355	
- below median SD		216		207
- above median SD		151		148
Late cohorts: New parents	673		657	
- below median SD		366		358
- above median SD		307		299
Early cohorts: Observations	3870		3309	
- below median SD		2211		1856
- above median SD		1659		1453
Late cohorts: Observations	6004		5393	
- below median SD		3246		2879
- above median SD		2758		2514
Tot observations	47549	47549	41762	41762

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 5, but it includes also estimates for the long-run (6 to 10 years after childbirth). The coefficients for the short run are exactly the same as in Table 5 because the estimated equation also includes the long run coefficients, which are however not displayed in that table.

Table A.17: The interaction between Paid Parental Leave and predictability (other variables)

	(1) Weekly hours in paid employment (condi-tional)	(2) Weekly hours in paid employment (condi-tional)	(3) Weekly hours in paid employment (condi-tional)	(4) Weekly hours in paid employment (condi-tional)	(5) Log(1+weekly wage)	(6) Log(1+weekly wage)	(7) Log(1+weekly wage)	(8) Log(1+weekly wage)
First child aged 0-5	-18.94***	-17.58***	-19.58***	-18.17***	-3.044***	-2.952***	-2.988***	-2.903***
	(0.845)	(1.072)	(1.065)	(1.281)	(0.161)	(0.207)	(0.192)	(0.246)
× above median SD predict.		-3.059+		-2.912+		-0.192		-0.184
		(1.719)		(1.728)		(0.329)		(0.329)
× not PPL eligible			1.399	1.111			-0.0984	-0.105
			(1.732)	(1.735)			(0.324)	(0.329)
First child aged 0-5 × First birth after 2010	3.774***	1.886	5.092***	3.229*	0.818***	0.763**	0.611**	0.551+
	(0.967)	(1.231)	(1.198)	(1.445)	(0.192)	(0.254)	(0.234)	(0.301)
× above median SD predict.		4.152*		3.823+		0.131		0.139
		(1.957)		(1.970)		(0.389)		(0.389)
× not PPL eligible			-3.321+	-2.996			0.400	0.406
			(2.015)	(2.013)			(0.388)	(0.393)
Observations	30279	30279	30279	30279	35482	35482	35482	35482
N Individuals	2451	2451	2451	2451	2454	2454	2454	2454
N New Parents	667	667	667	667	668	668	668	668

Standard errors in parentheses
$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 6, but estimates pertain to different labor market outcomes.

Table A.18: Child penalties in time use for Women

	(1) Playing with children (hrs/week)	(2) Housework (hours/week)	(3) Paid employment (hrs/week)	(4) Other activities (hrs/week)	(5) Physical activity (hrs/week)	(6) Social activity (hrs/week)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	45.78***	10.13***	-24.04***	-2.529***	-0.404***	0.607***
	(0.893)	(0.425)	(0.646)	(0.190)	(0.0616)	(0.0997)
First child aged 6-10=1	34.67***	10.71***	-21.91***	-2.025***	-0.257***	0.415***
	(0.916)	(0.518)	(0.806)	(0.271)	(0.0780)	(0.118)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	0.826	-1.976***	2.980***	0.0280	-0.0753	-0.232+
	(1.124)	(0.506)	(0.788)	(0.248)	(0.0774)	(0.123)
First child aged 6-10=1 × First birth after 2010=1	3.839**	-2.086**	2.841**	0.200	-0.199+	-0.215
	(1.342)	(0.691)	(1.061)	(0.418)	(0.115)	(0.164)
Pre-birth mean:						
Early cohorts	0.448	9.319	31.77	3.540	2.568	3.659
Late cohorts	0.395	7.707	32.45	4.509	2.657	3.374
Observations:						
N	92991	98187	96944	122094	105587	104702
N Individuals	10973	11125	11087	12879	11624	11595
N New Parents	1504	1513	1510	1539	1518	1517

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 7, but the child's age is split in coarser groups (two as opposed to four): first child aged 0-5 and 6-10.

Table A.19: Child penalties in time use for Men

	(1) Playing with children (hrs/week)	(2) Housework (hours/week)	(3) Paid employment (hrs/week)	(4) Other activities (hrs/week)	(5) Physical activity (hrs/week)	(6) Social activity (hrs/week)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-2=1	17.01***	0.321	-1.841**	-0.161	-0.306***	-0.430***
	(0.562)	(0.212)	(0.568)	(0.237)	(0.0746)	(0.104)
First child aged 3-5=1	16.37***	0.482*	-1.833**	-0.210	-0.355***	-0.435***
	(0.589)	(0.231)	(0.705)	(0.294)	(0.0870)	(0.111)
First child aged 6-8=1	14.67***	0.995***	-2.495**	0.0195	-0.239*	-0.389**
	(0.565)	(0.271)	(0.765)	(0.347)	(0.0944)	(0.124)
First child aged 9-10=1	13.89***	0.952**	-1.531+	0.0885	-0.188+	-0.256+
	(0.591)	(0.291)	(0.844)	(0.340)	(0.107)	(0.144)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-2=1 × First birth after 2010=1	0.0291	0.354	0.561	0.0447	-0.0196	0.161
	(0.710)	(0.267)	(0.719)	(0.291)	(0.0960)	(0.129)
First child aged 3-5=1 × First birth after 2010=1	1.489*	0.843**	0.395	-0.0327	-0.0207	0.0498
	(0.759)	(0.301)	(0.841)	(0.360)	(0.113)	(0.135)
First child aged 6-8=1 × First birth after 2010=1	0.973	0.613	0.434	0.0801	0.0282	0.0901
	(0.780)	(0.386)	(1.007)	(0.441)	(0.134)	(0.167)
First child aged 9-10=1 × First birth after 2010=1	-0.110	-0.378	-0.690	1.127	-0.175	0.119
	(1.370)	(0.634)	(1.338)	(0.926)	(0.235)	(0.291)
Pre-birth mean:						
Early cohorts	0.841	5.394	39.66	3.777	3.234	3.633
Late cohorts	0.597	5.234	39.90	4.599	3.192	3.289
Observations:						
N	82788	87063	86843	116055	93954	92971
N Individuals	10297	10425	10412	12748	10967	10915
N New Parents	1304	1309	1309	1338	1314	1310

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table 7 but estimates concern the subsample of fathers.

Table A.20: Child penalties in time use for Men

	(1) Playing with children (hrs/week)	(2) Housework (hours/week)	(3) Paid employment (hrs/week)	(4) Other activities (hrs/week)	(5) Physical activity (hrs/week)	(6) Social activity (hrs/week)
Panel A: Baseline Child Penalty (Early cohorts: 2003-2008)						
First child aged 0-5=1	16.71***	0.384*	-1.833**	-0.182	-0.327***	-0.431***
	(0.522)	(0.195)	(0.582)	(0.242)	(0.0718)	(0.0988)
First child aged 6-10=1	14.39***	0.939***	-2.120**	0.0521	-0.214*	-0.334**
	(0.527)	(0.256)	(0.739)	(0.312)	(0.0909)	(0.123)
Panel B: Change in Child Penalty (Difference of Late cohorts (2010-2017) from Early cohorts)						
First child aged 0-5=1 × First birth after 2010=1	0.647	0.522*	0.482	0.0207	-0.0163	0.118
	(0.656)	(0.247)	(0.706)	(0.289)	(0.0917)	(0.120)
First child aged 6-10=1 × First birth after 2010=1	1.026	0.483	0.0373	0.144	-0.00203	0.0608
	(0.740)	(0.367)	(0.963)	(0.408)	(0.128)	(0.165)
Pre-birth mean:						
Early cohorts	0.841	5.394	39.66	3.777	3.234	3.633
Late cohorts	0.597	5.234	39.90	4.599	3.192	3.289
Observations:						
N	82788	87063	86843	116055	93954	92971
N Individuals	10297	10425	10412	12748	10967	10915
N New Parents	1304	1309	1309	1338	1314	1310

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: This table is analogous to Table A.18 but estimates concern the subsample of fathers.

B Variables construction

Demographics and labor market outcomes "Age" is the age of the respondent. "In labor force" is a dummy equal to one if the individual is either employed or unemployed but looking for work, and equal to zero if the individual is not in the labor force; it can be missing if the individual is not administered this part of questionnaire - see Summerfield et al. (2019) for further details. "Weekly hours in paid employment" is conditional on hours being strictly positive. "Log weekly hours in paid employment" is the natural logarithm of the variable above. "Log(1+ weekly wage)" is the log of 1 plus weekly wages, in contemporaneous Australian dollars, and is winsorized at 0.5% level, by year and gender. "Log(1+ annual labor income)" is the log of one plus yearly earnings, similarly winsorized.

The way hourly wages are measured and computed in Hilda might not be reliable in periods of large job changes (such as after childbirth for example) because of the way the questions are worded. In fact, hourly wages are computed as the ratio of weekly wages to the number of hours spent in paid employment every week. However, weekly wages are computed from a question that asks "What was the total amount of your most recent pay" (suggesting to take out a pay slip to answer accurately) while weekly hours come from a question that asks "Including any paid or unpaid overtime, how many hours per week do you usually work in all your jobs?". Moreover, they are asked in two different parts of the survey (questions about income come much after other questions on current employment). Therefore, while for a person that has not experienced large job changes we can expect the answers to the two questions to be consistent, we cannot have the same expectation for people who are experiencing large job changes, such as new mothers. For example, we might expect a new mother to answer the wage question based on the pay slip she received when she last worked while the hours questions based on the hours she works now. Or, alternatively, she might be counting the amount she receives for paid leave as wages. In any case, at least for the first year of life of the child hourly wage is not a reliable measure. Therefore we follow the suggestion from Summerfield et al. (2019) and construct our variable "Hourly wage" using a measure of weekly wages that combines current weekly gross wages salary in all jobs (winsorised as described above. "Part-time if employed" is a dummy variable equal to one if the individual is employed and working part-time, and equal to zero if the individual is working full-time (it is missing for unemployed individuals)."Permanent employment if employed" is a dummy equal to one if the respondent has a permanent contract, and equal to 0 if they have casual or fixed-term contracts (it is missing for unemployed individuals). The two "Overtime" variables are dummies equal to one if the respondent works more than 38 or 50 hours a week (respectively), and zero otherwise (it is missing for unemployed individuals). "Promoted in the past year" is a dummy equal to one if the individual is currently employed and has been promoted in the previous year, and zero otherwise (it is missing for unemployed individuals). "Normally supervise work of other employees" is a dummy variable equal to one if the respondent has supervisory responsibilities (it is missing for unemployed individuals). "Occupational status scale" is the Australian Socioeconomic Index 2006 from McMillan et al. (2009), ranging from 1 to 100. "Regular schedule (M-F and regular daytime schedule) is a dummy equal to one if the individual has both a regular Monday-Friday weekly schedule and a regular daytime daily schedule. "M-F" and "Regular daytime schedule" are the two corresponding dummies. "On call and irregular shifts" is a dummy equal to 1 if the individual's current work schedule is either call-based or irregular. "My working times can be flexible" is built based on a categorical variable denoting how strongly respondents agree, on a scale from 1 to 7, with the statement "My working times can be flexible". We discretize it by indicating as agreement any answer above 4 and disagreement otherwise. "Entitled to flexible start/finish times" is a dummy variable equal to 1 if the respondent answers yes to the question "Would you be entitled to flexible start/finish times if you were to need it?". "Has bachelor degree" is a dummy variable equal to one if the respondent holds a bachelor degree, a graduate diploma or a postgraduate one.

Workplace flexibility The first measure of workplace flexibility we adopt is a dummy indicating agreement with the statement "My working times an be flexible". This is built based on a categorical variable denoting how strongly respondents agree, on a scale from 1 to 7, with the statement “My working times can be flexible”. We discretize it by indicating as agreement any answer above 4 and disagreement otherwise. Results are unchanged if we use the categories separately.

The second measure of workplace flexibility we adopt is the answer to the question “Would you be entitled to flexible start/finish times if you were to need it?".

Predictability There are mainly two questions that refer to predictability. The first one, which we label “Regular weekly schedule" asks “On which days of the week do you usually work in your main job?", and possible answers are “Monday to Friday", “Nine day fortnight", that their days vary from week to week, and that they vary from month to month.

The second question is “Regular daily schedule", and respondents are asked “Which of these best describes your current work schedule in your main job?". Possible answers are listed below.

1. A regular daytime schedule
2. A regular evening shift
3. A regular night shift
4. A rotating shift (changes from days to evenings to nights)
5. Split shift (two distinct periods each day)
6. On call
7. Irregular schedule
8. Other

Our measure, which is called “Regular schedule" is a dummy variable equal to 1 if the answer to the first question is “Monday to Friday" and the answer to the second question is “ A regular daytime schedule", and equal to 0 for any other combination of the two answers.

Work from home The variable “Hours worked from home" is based on the question “Approximately how many hours each week do you usually work at home?". This is missing for unemployed respondents. The variable “Any hours worked from home" is a dummy variable equal to one if the variable defined above is strictly positive. The variable “Hours worked from home conditional on any" is equal to “Hours worked from home" for those who work from home a strictly positive number of hours, and missing otherwise. The variable “Entitled to home-based work" is a dummy variable equal to one if the respondent answers yes to the question “Would you be entitled to home-based work if you were to need it?"

Time use For most of our time use variables we rely on direct questions of the form “How much time would you spend on each of the following activities in a typical week?" This is true for the following measures:

- Playing with your children
- Housework
- Paid employment

- Other (which aggregates commuting, volunteering, looking after other people's children, caring for disabled spouse)

The variable "Social activity" is constructed from the question "In general, about how often do you get together socially with friends or relatives not living with you?" with the following possible answers:

- Every day
- Several times a week
- About once a week
- 2 or 3 times a month
- About once a month
- Once or twice every three months
- Less often than once every three months

We assume that each "get together" occasion lasts for about two hours and therefore we recode the above as follows:

- Every day – 14 hours per week
- Several times a week – 6 hours per week
- About once a week – 2 hours per week
- 2 or 3 times a month – 1.25 hours per week
- About once a month – 0.5 hours per week
- Once or twice every three months – 0.15 hours per week
- Less often than once every three months – 0 hours per week

For "Physical activity" we proceed similarly. The questionnaire asks "In general, how often do you participate in moderate or intensive physical activity for at least 30 minutes?", with the following possible answers:

- Not at all
- Less than once a week
- 1 to 2 times a week
- 3 times a week
- More than 3 times a week
- Every day

We assume that each sports session lasts one hour and we recode the variable as follow:

- Not at all – 0 hours per week
- Less than once a week – 0.8 hours per week

- 1 to 2 times a week – 1.5 hours per week
- 3 times a week – 3 hours per week
- More than 3 times a week – 4.5 hours per week
- Every day – 7 hours per week